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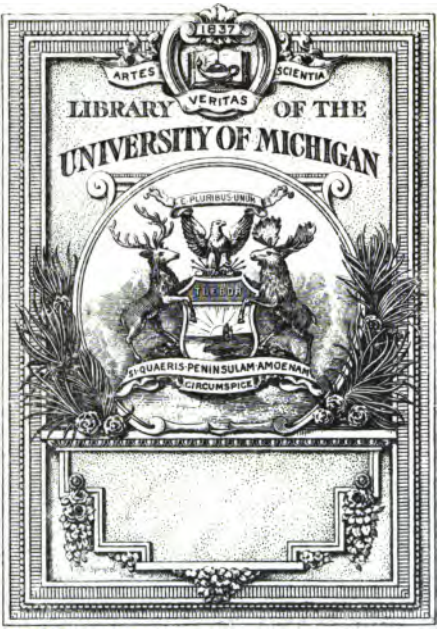
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BURNETT LECTURES.

ON LIGHT.

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ON LIGHT.

Second Course,

ON LIGHT AS A MEANS OF INVESTIGATION.

DELIVERED AT ABERDEEN IN DECEMBER, 1884,

BY

Sir GEORGE GABRIEL STOKES, M.A., F.R.S., &c.

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ON LIGHT AS A MEANS OF INVESTIGATION.

LECTURE I.

Subjects of the present course—Use of the mode of absorption of Light by substances as a discriminating character—Examples—Various effects produced by the action of Light incident on bodies—Phosphorescence—Epipolic dispersion of Light—Fluorescence—Its use as a means of discrimination—Phosphorescence produced by electric bombardment—Delicate test of Yttria thus afforded.

IN the course of lectures which I had the honour of delivering to you last year, I dwelt on the nature of light itself, and endeavoured to give you a fair idea of the evidence on which we accept that view of its nature which is now I may say almost universally held by scientific men, namely, that light consists in undulations propagated in a highly elastic medium called the ether, and that those vibrations are not, as we should previously have imagined, to and fro in the direction of propagation, as we know are those of air in the propagation of sound, but transverse to the direction of propagation. At the conclusion of the course I stated that according to an arrangement adopted in consultation with the Burnett Trustees the subject of the present year's course should be investigations carried on by the aid of light; or in other words on light as a means of investigation.

In one sense of course this would include nearly every investigation that we can carry on; for in nearly every case we make use of our eyes, and without light they would be of no avail. But it is obvious that it cannot be in this comprehensive sense that the title of the present course is meant to be taken. The investigations actually intended are those in which the properties of light in their relation to ponderable matter enable us to ascertain something regarding the nature or the condition of that ponderable matter. Even as thus restricted the subject remains a wide one, branching out into other departments of science, especially chemistry, mineralogy and astronomy.

The special subjects belonging to our general class which I have selected to bring before you are (1) absorption, and its application to the discrimination of bodies; (2) the emission of light consequent on absorption, or produced by different means other than incandescence, and its application as a test of the presence of particular bodies, or of the condition of the bodies so emitting it; (3) the rotation of the plane of polarization of polarized light, and its connexion with the constitution of bodies; (4) the emission of light by incandescent bodies in the state of vapour, and its application as a test of the presence of particular bodies; (5) the information thus afforded as to the constitution or condition of distant bodies; (6) the influence of the motion of bodies on the refrangibility of the light emitted, absorbed, or reflected by them, and the information thence afforded as to the motion of distant bodies.

That colours in all cases depend primarily on the heterogeneous nature of light, on its consisting of kinds differing from one another in refrangibility, and in the sensation of colour which they produce when separated one from the other, was long ago shown by Newton. In all cases the existence of colour depends either on the emission of a certain kind or certain kinds of light in excessive proportion in the source of light, as in the case of coloured flames, or in a subsequent treatment of the light of such a nature that certain kinds are preserved for reception by the eye to the exclusion of others, or preserved in larger proportion than others. The production of colour by fluorescence, of which more presently, forms no exception to this rule provided we regard, as we may regard, the fluorescent body as the source of light, albeit in this case a secondary source.

By far the commonest exhibition of colour belongs to what is called absorption. This applies for instance to the verdure of the fields, the colours of flowers, those of dyed dresses, &c.

If we take for example a coloured flower, and hold it in different parts in succession of a pure spectrum, it appears of the colour of the part of the spectrum in which it is placed, but its luminosity varies greatly from one part of the spectrum to another, the flower appearing comparatively bright in those colours which approximately agree with that under which it is seen in white light, and comparatively dark, and sometimes almost black, in other parts of the spectrum. This shows, as Newton pointed out, that the proximate cause of the exhibition of colour in ordinary light is that the flower reflects copiously light of certain kinds, and reflects feebly or hardly at all light of other kinds.

But what is the nature of the selection of certain colours for reflection while others are not reflected? Newton's attempt to explain this by reference to the colours of thin plates was not a happy one, and the inadequacy of such an explanation has long been recognised.

To illustrate the true answer let us take one of the commonest of all colours, the green of vegetation. If a green leaf be put into alcohol—a plant with an acid juice, like sorrel, should be avoided, as in that case special treatment is required to avoid decomposition—the green colouring matter is dissolved, and we obtain a beautiful clear green solution. [This was shown.] There cannot be any doubt that the cause of the colour in the leaf and in the solution is the same, but in the solution it is quite plain that it is in the *transmitted* light that the colour is seen. If we examine the behaviour of the liquid with respect to any particular kind of light, we find that as the light travels onwards in the solution it continually grows weaker and weaker, and presently becomes too weak to be any longer perceived. It is found that in passing across a layer of the liquid of given thickness a given fraction of the light is lost, from whence it readily follows that as the light travels on in the solution its intensity decreases in geometric progression as the distance traversed increases in arithmetic progression. But the rate of loss varies immensely from one kind of light to another. For one kind there may be hardly any loss at all: the liquid may behave almost like water; while for another kind the absorption may be very rapid, and the liquid may behave almost like ink. For other kinds again the rate of absorption may be intermediate, and the liquid behave like water with some ink mixed with it.

Our clear solution requires to be looked through in order that the colour may be seen; for if it be looked at, and the colour seen by means of light which has been reflected from the further side of the vessel, that comes to the same thing.

Now suppose we mix some flour or powdered chalk with the liquid. We shall be able to see the colour by looking at the liquid mixture; [This was shown.] but that is evidently because the white powder reflects light from its surfaces in an irregular manner, and the light so reflected has had to pass through the solution in the interstices between the reflecting surfaces before it reaches the eye. It is just the same thing in a natural leaf; the irregularities of the structure cause irregular reflections and refractions of light in the substance of the leaf, and the light in its progress is exposed to absorption on part of the colouring matter.

The colour of solutions of metallic salts or of organic substances has naturally long been used as a means of discrimination. But the eye can only very imperfectly judge of the composition of a mixed light by means of the colour. When the mixture is resolved into its constituents by means of a prism, it can then be seen at a glance how each constituent kind of light has been affected.

Now in many cases some two or more parts of the spectrum are specially attacked by the coloured substance, and when that is the case the character and position of the bands of absorption when the light has been strained by passing through a suitable thickness of the coloured substance, which is usually most conveniently examined in the condition of a solution, or of a clear glass, are often highly distinctive. Till of late years, chemists were unaware of the simple means of discrimination thus afforded, and occasionally made mistakes which a single glance at the absorption spectrum of the substance they had under examination would have prevented. Physicists indeed were familiar with these phenomena, but they usually studied them with other objects in view. Now, in consequence of the labours of Bunsen and Kirchoff, a spectroscope of some kind is considered an indispensable portion of the equipment of a chemical laboratory.

I will mention two or three examples. Under certain circumstances red solutions are obtained by the action of acids or acid salts on certain oxides of manganese, which resemble a good deal in colour the permanganates, and which like them are oxidising agents. It seemed not unnatural to infer that the colour of the former class of solutions was due to permanganic acid, and a paper was written in one of the scientific journals in support of this view. A single glance at the spectrum of the transmitted light would have shown this to be an error, for the permanganates are characterised by a remarkable and highly distinctive system of bands of absorption, of which there are five occupying the region of the brightest part of the spectrum. These are wholly wanting in the spectrum of the solutions of the class first mentioned.

A long time ago the eminent chemist M. Fremy attempted to divide chlorophyll, as the colouring matter of green leaves is called, into a blue and a yellow substance, of which it was a mixture. He attempted to carry out this idea in two ways. In one he added to the alcoholic solution hydrate of alumina and a very little water, and filtered. The filtrate was of a yellow colour, and the precipitate on being treated with appropriate solvents for the colouring matter it contained yielded solutions which were not indeed blue, but of a decidedly bluer green than before. In another attempt he dissolved chlorophyll in a mixture of hydrochloric acid and ether, when on separation of the two solvents an upper yellow stratum was obtained and an under one which was nearly blue. Fremy designated the colouring matters contained in the yellow solutions obtained in these two ways by the same name phyxoxanthine; whereas the prism shows that the first is a definite colouring matter characterised by two bands of absorption in the blue, but the yellow solution obtained by the ether process is a complex mixture.

In the detection and separation of the rare earths of the Ceria and Yttria groups, the number of which has recently been largely increased, and which chemists are now actively engaged in investigating, great assistance is derived from the very peculiar spectra which the solutions of the salts of some of them present. Dr Gladstone was I believe the first to point out the peculiar spectrum of the salts of didymium, which show bands of absorption almost rivalling those of coloured gases in their narrowness.

This brief sketch may serve to give some idea of the way in which the relations of light to ponderable matter may be turned to account in investigations belonging to a branch of science altogether different; though instances far more striking will fall under our notice in a future lecture. I pass on now to a different though allied subject.

In the phenomenon of absorption, as the very name indicates, light is as it were swallowed up and disappears. But, as belonging to the incident light, a certain amount of energy is continually being brought to bear on the body in which the light is absorbed, and this energy cannot be annihilated; there must be something to show for it.

Now different effects are produced in different cases, or it may be are simultaneously produced in the same instance. The commonest effect of all, one indeed that would seem to be always present, whether alone, or mixed with others, is that of raising the temperature of the body on which the light falls. It is true that the rays which are the most luminous are by no means those by which this effect is most powerfully produced; and that they are far surpassed in this respect by certain rays lying beyond the red, which though physically identical in their nature with rays of light, from which they differ only in the same way in which rays of one colour differ from those of another, do not nevertheless affect the eye. Still, the effect is produced by rays of whatever refrangibility, though feebly it is true by those of high refrangibility. But this effect of absorption, though the commonest of all, is not of a nature to be made available as a means of discrimination.

Another effect sometimes produced is that of effecting chemical changes. On this is founded the whole practice of photography. Although the seat of the chemical change varies from one part of the spectrum to another, with a variation in the chemical nature of the substance acted on, the variation cannot, except in rare cases, be conveniently used as a means of discrimination. Photography may indeed be most usefully employed for the purpose of detecting and recording what is going on in different parts of the spectrum, especially in the case of the ultra-violet and ultra-red regions, where eye observations fail. But this does not properly come under the subject of the present course.

Another effect sometimes produced as a result of the absorption of incident rays of light is that of phosphorescence or fluorescence. It has long been known that certain precious stones, especially the diamond, and the sulphides of the alcaline earths after exposure to light shine for some time in the dark.

[A collection of phosphorescent sulphides was shown, which glowed with different colours after having been excited by the radiation from burning magnesium wire.]

The laws of this phenomenon have been investigated by many physicists, especially by M. Edmond Becquerel, who has now given a collected account of his labours and those of his predecessors in his work entitled *La Lumière.* This phenomenon forms no exception to the statement I have made, that in the phenomenon of absorption the incident light is swallowed up and disappears; for though it is true that as a result of that absorption light is forthcoming, yet that light is not in any way of the character of the incident light, but of a different composition altogether. The phosphorescent body is rendered for a time self-luminous as a result of the action of the light upon it, but the incident light itself is spent in the process.

This phosphorescence of long duration has not however been much used as a means of distinguishing one body from another, with one remarkable exception, in the case of a similar effect produced in a different way, to be mentioned presently.

There is however a phenomenon which appears to differ from phosphorescence only in degree, in point of duration, which is more generally available for this purpose, at least in the case of organic bodies.

In the course of some experiments on the action of light on vegetable colours, in which he had occasion to throw a more or less pure spectrum on the substance to be examined, Sir John Herschel noticed a remarkable prolongation of the spectrum beyond the violet, which is usually regarded as the termination of the visible spectrum, when the spectrum was thrown on turmeric paper and a few other substances. This extension Sir John Herschel attributed to a peculiarity in the reflecting power of the paper: and as, in the case of turmeric paper, which showed the phenomenon best among the substances which he tried, the prolongation was coloured yellow, he even speculated on a possible repetition of the colours of the spectrum in order after passing beyond the violet.

Some years later he observed a curious phenomenon in a solution of sulphate of quinine of moderate strength. This liquid when seen by transmitted light appears colourless and transparent like water, but when viewed otherwise exhibits in certain aspects a peculiar blue colour. This, Herschel found, proceeded from a narrow stratum of the liquid adjacent to the surface by which the light entered, though a small part came from greater depths; and the blue light emanated from this stratum in all directions. But the most remarkable thing was that the light which had once produced this effect, though not apparently altered by transmission through the liquid, was deprived of the power of producing the effect; so that the light which had passed through a cell filled with the liquid, and then fell on another vessel containing a similar solution, no longer produced a blue stratum near the surface by which it entered the second solution. Herschel called the phenomenon of the production of the blue stratum *epipolic dispersion*, and designated as *epipolised* the light which, by passage through such a solution, had been deprived of the power of producing the blue stratum again, though he did not explain wherein epipolised differed from common light.

I have already alluded to the careful study made by M. Edmond Becquerel of the phenomenon of phosphorescence. While engaged in this research he noticed that when a pure spectrum was allowed to fall on several of the phosphori examined (especially sulphides of the metals of the alcaline earths), in certain regions of the spectrum, whether belonging to the visible part or to the invisible region of greater refrangibility, the phosphorus shone with a light of a different kind from that which fell upon it, but only so long as the light remained falling upon it, differing in this respect from the ordinary phenomena of phosphorescence, in which the phosphorescent light lasts a very appreciable time. This phenomenon he rightly regarded as a phosphorescence of very brief duration; but from connecting it too closely with ordinary phosphorescence he failed to perceive its full bearing; and though he was actually working with an acid solution of quinine, the "dichroism" of which he expressly mentions, and had determined by photography its great opacity for rays of high refrangibility, he never thought of putting these things together, or perceived their intimate connexion with the phenomenon just mentioned of phosphorescence of short duration.

In reflecting on the possible explanation of the epipolic analysis of light discovered by Sir John Herschel, I was led to believe that the rays which produced the blue light dispersed in a solution of sulphate of quinine were not the blue rays at all, but the rays of high refrangibility which are mostly invisible. Once this idea is suggested it is easily put to the test of experiment, and the result completely verified the anticipation.

Perhaps the most striking feature in this phenomenon is the change of refrangibility of light which takes place in it, as a result of which visible light can be got out of invisible light, if such an expression may be allowed: that is, out of radiations which are of the same physical nature as light, but are of higher refrangibility than those that affect the eye; and in the same way light of one kind can be got out of light of another, as in the case for instance of an alcoholic solution of the green colouring matter of leaves, which emits a blood red light under the influence of the indigo and other rays. Observation shows that this change is always in the direction of a lowering.

But in speaking of a change of refrangibility I would guard against being misunderstood. All that is intended is that light of one refrangibility being incident on the substance, light of a different refrangibility is emitted so long as the first remains in action. It is not to be supposed, according to a view which has erroneously been attributed to me by more than one writer, but which I never for a moment entertained, much less published, that the refrangibility is changed in the act of reflection from the molecules. The view which I have all along maintained is that the incident vibrations caused an agitation among the ultimate molecules of the body, and that these acted as centres of disturbance to the surrounding ether, the disturbance lasting for a time which, whether it was long enough to be rendered sensible in observation or not, was at any rate very long compared with the time of a single luminous vibration. And now that M. E. Becquerel has shown experimentally by his beautiful phosphoroscope the finiteness of duration of the emission of light in the case of solids in which it was so brief that its emission was described as "fluorescence," as in a solution of sulphate of quinine, there can no longer be any doubt as to the identity of nature of phosphorescence and fluorescence, even though the finite duration of the emission of light after the incident rays have been cut off has not at present been experimentally demonstrated in the case of any liquid.

I will not dwell further on the nature of this phenomenon, because the subject of the present course mainly confines me to the consideration of light as a means of research. In this point of view it is obvious that as the presence or absence of invisible rays of high refrangibility in a pure spectrum is so easily shown by the exhibition or non-exhibition of fluorescence in a suitably chosen fluorescent body, the phenomenon renders these rays virtually visible, and enables us thereby at once to study the action of bodies upon them, such as the absorbing power of bodies for them. This however constitutes rather an extension of another optical means of discrimination than an independent method. But even as an independent means of discrimination the phenomenon is by no means devoid of application. The property is sufficiently common to be pretty frequently available, and yet sufficiently uncommon to be, at least when taken in all its features, highly distinctive. Experience shows that at least in the case of a single fluorescent substance, as distinguished from a mixture of two or more such substances, the tint of the light emitted, with a given solvent if the active substance be in solution, is usually sensibly constant whatever may be the refrangibility of the active rays. The tint admits of being observed by suitable methods notwithstanding the admixture of various other bodies with the one in question, even though they should be coloured, provided they are not themselves fluorescent. In this respect it is somewhat superior to the colour due to absorption, the observation of which requires that the coloured body should be at least approximately isolated, so far at least as coloured substances are concerned.

As an example of the use that may be made of the observation of the tint of the fluorescent light, I may refer to a powerfully fluorescent and easily obtained solution which may be made from the bark of the horse chestnut. A decoction of the bark is powerfully fluorescent, as may best be seen by pouring a little into water, but it is liable soon to become brown from tannin &c. contained in it. This may be removed by adding to the freshly made solution a suitable metallic salt, such as a salt of alumina or sesqui-oxide of iron, precipitating by ammonia, and filtering, when the filtrate shows the phenomenon to perfection. Long before the true nature of the phenomenon was known, a glucoside named aesculin had been obtained from the bark, to which the play of colour was attributed. But similar solutions of aesculin show a fluorescence of a decidedly deeper blue than that of the solution obtained directly from the bark. This shows either that the aesculin is a product of decomposition—a hypothesis which is negatived by the fact that the reagents employed do not affect the tint of the fluorescent light of the solution obtained from the bark—or that in the latter the aesculin must be mixed with some other fluorescent body. In fact the bark of the horse chestnut contains a second glucoside analogous to aesculin, and yielding like it highly fluorescent solutions, which has been named fraxin from its having been first discovered in the bark of the ash. The slightly alkaline solutions of fraxin show a fluorescence which is intermediate between blue and green, and the presence of both bodies in the solution obtained from the bark explains the greater paleness of the blue of the light emitted from it by fluorescence than of that coming from a solution of pure aesculin.

[The fluorescence of solutions of pure aesculin and fraxin obtained from the bark of the horse chestnut, and of a purified solution of the bark itself without any separation of the two bodies, were exhibited by holding burning magnesium wire over glasses containing the three solutions.]

I have already mentioned the searching character of prismatic analysis when applied to the examination of absorption-colours, as compared with a mere examination of the colours by the naked eye. Just in the same way the observation of fluorescent substances in a pure spectrum exhibits features by which they may be followed and detected in spite of the presence of other substances even in large quantity.

In proceeding in the order of increasing refrangibility, that is from the red to the violet and beyond, the fluorescence is found to commence at a part of the spectrum differing with the particular substance observed, and once commenced to continue from thence onwards. It is frequently however, indeed I might almost say generally, subject to fluctuations of intensity; in one place the fluorescence being copious, and the rays which excited it being soon spent, while in another place it is comparatively feeble, and the rays which excited it, unless they should happen to be absorbed by some other substance present in an impure mixture, are able to penetrate to comparatively great distances in the solution before they are spent. Both these, the place of commencement and the copiousness of emission, form discriminating characters which may be usefully employed. The first is however very much bound up with the colour of the emitted light, and therefore hardly forms an *independent* feature, except in the case of a mixture of two or more fluorescent substances, but in this case is often of great utility.

The second again is bound up with the character of the absorption due to the substance, so that it hardly forms an independent feature when the latter can be observed; but it has this advantage over absorption as a discriminating character, that whereas the latter requires the substance to be approximately isolated from other coloured substances in order that it may be observed, the former can be observed independently in great measure of the presence of such impurities, and enables us in fact to predict the character which the absorption-spectrum of the substance will exhibit when isolated.

I will not dwell further on these phenomena of fluorescence, which are too much of a speciality to be of very general interest. But before I leave the subject of phosphorescence there is one research which I must mention, to which allusion has already been made.

In the instances of phosphorescence, including that phosphorescence of brief duration denominated fluorescence, which have hitherto been mentioned, the phosphorescence was excited by light, or by radiations of the same physical nature as light, though they might not be capable of affecting the eye. But there is another mode of exciting phosphorescence which has been much studied by Mr Crookes and some others. Electric light in all its forms, abounding as it does in rays of high refrangibility, is specially well suited to excite phosphorescence in most phosphorescent substances. In particular, the beautiful discharge in pretty highly exhausted tubes is well adapted to excite it on bodies placed outside, or still better when that is practicable on bodies within the tube, or on the walls of the tube itself. So far, we have merely a particular case instance of the ordinary mode of exciting phosphorescence. But when the exhaustion is very high a new mode of exciting it comes into action.

We are familiar with the glow surrounding the negative electrode in tubes which are considerably exhausted. When the exhaustion is moderate the glow appears to invest closely the negative electrode. But as the exhaustion progresses it is seen to be separated from the electrode by a dark space, which becomes wider and wider as the residual gas becomes rarer and rarer, until at last it reaches the walls of the tube, and may even at extreme exhaustions fill the whole tube. Now the portion of the walls which lies within the dark space is seen to glow with a phosphorescent light resembling in colour the phosphorescence produced by radiation from the gas glowing under the influence of the discharge with more moderate exhaustions, but usually a good deal brighter; and the same is the case with substances placed within the tube when the dark space extends over them. Experiments instituted with the direct object of determining whether the phosphorescence in this case is due to radiation or to an actual bombardment of the walls or enclosed substances by molecules projected with great velocity from the negative electrode indicate that the phosphorescence is to be referred to bombardment, provided at least that we do not by the use of that term imply that the action is merely mechanical, but only assert that it is due to the actual transfer, or something accompanying the actual transfer, of the molecules; for it seems probable that the electric discharge, whatever the appropriate idea of that may be, has much to do with it in a direct way. If in this wider sense of the term the phosphorescence is really due to bombardment, that justifies us in speaking of it as a distinct mode of production from the ordinary one in which it is due to radiation.

From the processes to which the tube has to be subjected in order to obtain exhaustions so nearly perfect as is requisite for this observation, the method is pretty well confined to the examination of inorganic substances. In studying the spectra of the transmitted light, Mr Crookes frequently came across a characteristic citron band, evidently indicative of some particular substance. Was it a new element, or rather compound of a new element, or was it a compound of one of the known elements, including therein a number of new and rare earths recently discovered and as yet imperfectly known? The known elements were tried, and for a long time apparently with a negative result. Frequently the substance Mr Crookes was in search of seemed to be driven into a corner, and yet it managed to slip through the fingers. At last the loophole was discovered, and the substance proved to be the long known but rare earth yttria, in combination with sulphuric acid. Once the origin of the citron band was known, it furnished a very delicate test of the presence of yttria, and the application of this test showed that this rare earth is in reality very widely distributed, though in general in minute quantity. This illustrates what is I believe generally admitted by chemists, that chemical purity represents an unattainable ideal, to which we can only make a more or less near approach in actual experiment.

LECTURE II.

Rotation of the plane of polarization of polarized light produced by various liquids—Its application to quantitative determinations, and to the study of molecular grouping—Magnetic rotation of the plane of polarization—Application to the discrimination between isomeric compounds—Bright lines in the spectra of flames—Application as chemical tests—Discovery thereby of new elements—Connexion between the powers of emission and absorption of the same substance for the same kind of Light—Conditions as to temperature which determine whether a spectral line shall appear as bright or dark.

I HAVE mentioned the rotation of the plane of polarization of polarized light as another property of light which has been turned to account in investigation. It was in the year 1815 that Biot, in pursuing his investigations of the colours developed in polarized light which is subsequently analysed, by the introduction of a crystalline plate which is variously inclined, being desirous of working with inclinations within the crystal greater than could be obtained when the plate is inclined in air, was led to try the effect of inclining the plate when immersed in oil of turpentine contained in a cell with parallel sides. The oil being of considerable refractive index, approaching that of plate glass, and accordingly that of ordinary minerals, would allow the passage of light within the crystal at an inclination higher than that attained at even a grazing incidence when the plate is in air. But in order to establish the legitimacy of the process it was necessary to show that the oil did not itself act on polarized light. Contrary to what was to have been anticipated, inasmuch as the oil is alike in all directions, differing notably in that respect from a crystal, the oil *did* act on polarized light, and Biot was thus led to the discovery of the action of certain liquids on polarized light. This affords a striking example, of which there are so many, of the way in which the honest pursuit of scientific investigations will sometimes lead to a discovery unthought of by the person who made the experiment with a totally different train of ideas in his mind.

The amount of rotation is found to be different for the different colours, being greater for the more refrangible ones. Accordingly the transmitted light is not wholly extinguished in any position of the analyser, but the different colours are extinguished in succession, so that on rotating the analyser there is a constant change of colour.

For polarized light of a given refrangibility passing through an active liquid of given kind, the rotation is found to be proportional to the length of path of the light within the liquid, as might have been anticipated. When the liquid is not homogeneous, but consists of a solution of an active substance in an inactive solvent, the rotation is found to be proportional to the strength of the solution. Hence the observed rotation divided by the length of path and by the strength of the solution, is a constant depending on the nature of the active substance, and may be called the specific rotation. The specific rotation, like any other physical constant belonging to the substance, may be used as one of the characters by which it may be known, but its chief value arises in part from the comparative rarity of the rotatory property, so that in many cases there is but one such body liable to be present in the solution, which in that case may be determined quantitatively notwithstanding the presence of other substances ; in part from the delicacy of the molecular combinations of which it is able to take cognizance, and of the existence of which it sometimes constitutes the sole evidence.

Let me mention an example of each kind of application. One of the commonest substances which in inactive solvents yield active solutions, and one which at the same time possesses a high specific rotation, is sugar in its different forms. When sugar is the only active substance liable to be present, and the form in which it occurs is known, we can by simply observing the rotation of the plane of polarization produced by passage of the light through a known length of the solution determine the quantity of sugar present. Accordingly an instrument designed for effecting this measurement with accuracy and facility has been named a saccharimeter.

A rather amusing application of the saccharimeter which has been proposed and I believe actually employed in some foreign country is for excise purposes, in fixing the duty on beer, which according to a law there in force depends on its alcoholic strength. In distilled spirit we have practically only two substances liable to be present, namely, water and alcohol, which are of very different specific gravities, so that the specific gravity of the mixture, which can be easily and rapidly taken by the hydrometer, determines the strength. But in beer the specific gravity depends not only on the alcohol, which lowers it, but on the sugar, or rather dextrine, which raises it. (The dissolved substances other than sugar may be neglected, as being present in no great quantity.) Hence the specific gravity gives only one relation between two unknown quantities. But the amount of sugar can be determined by measuring the rotation of the plane of polarization, and knowing this we can calculate what the specific gravity would become if the sugar were removed, and from thence deduce the alcoholic strength.

As an instance of feeling, so to speak, a delicate molecular combination by the observation of the azimuth of the plane of polarization of polarized light, I will refer to the behaviour of a freshly made solution of grape sugar or glucose. This substance crystallizes by itself, i.e. with merely water of crystallization, and curiously enough forms also a definite crystalline compound with common salt. The crystals of either kind dissolve readily in water, and the solutions when once made remain apparently unchanged. Nevertheless when the rotation of the plane of polarization produced by the solution of either kind of crystals is immediately measured, it is found to be nearly double that of a solution of glucose of the same strength which has been made some time. The rotatory power of the fresh solution gradually diminishes, and in the course of seven hours or so at ordinary temperatures reaches a permanent value, a change which takes place at once on boiling. Had this change occurred only in the solution of the crystallized compound of glucose and common salt, we should naturally have inferred that the compound at first dissolved as such, but was of an unstable character, and this may possibly actually be the case; only if it be so some analogous change must take place when we have nothing present but glucose and water, and what the nature of the change of molecular grouping in that case may be we do not know. It is remarkable that cane sugar, though so closely allied to glucose, shows no such phenomenon.

As another example of the application of this method to the study of the mode of combination of bodies in mixed solutions, I may refer to an elaborate research by Dr Jellett, on the combinations formed when a vegetable alcaloid is present in a solution containing two acids, with either of which the alcaloid could combine.

It was in the year 1845 that Faraday made the remarkable discovery that uncrystalline bodies, or at least those of high refractive power, when under the influence of a powerful magnetic force act on polarised light. The action consists in a rotation of the plane of polarization, agreeing so far with the natural action of liquids like sirup of sugar, from which however it differs in the circumstance that whereas in the latter the action is alike in all directions, in the former the amount of rotation varies with the direction of the light. It is greatest when the light travels in the direction of the lines of magnetic force, and vanishes in a direction perpendicular to those lines, and the direction of rotation is reversed on passing across the equatorial plane, or plane perpendicular to the lines of magnetic force.

It has been found that for a given substance and kind of light the rotation is proportional to the length of path, to the magnetic force, and to the cosine of the inclination of the path to the lines of force, from whence by measuring the rotation under given circumstances we can determine the specific rotation, or more readily the ratio of the specific rotations of two substances—more readily because in this case we do not require the magnetic field to be sensibly uniform (or else calculable, so as to permit us to perform an integration) and the magnetic force known; it is sufficient that the magnetic field and the force in it be the same in successive experiments.

The specific magnetic rotation, like the specific gravity, the refractive power, &c. is a physical constant giving one of the characters of each particular substance. An elaborate comparison of the specific rotations of several chemical groups has recently been made by Mr Perkin, with the result among others that the difference between isomeric compounds is in many cases clearly revealed by a difference in the values of *this* constant, whereas most other physical constants are nearly alike for the two.

It appears therefore that the rotation of the plane of polarization produced by the action of magnetism on bodies across which light is proceeding, like the natural rotation belonging to such bodies as sirup of sugar, &c. is capable of laying hold of and revealing delicate differences of molecular grouping. It is less easily observed than the natural rotation, from which also it differs in being of general instead of exceptional application.

In all the phenomena which I have brought before you in my last lecture and in this, and indeed in all that I shall have occasion to mention in this year's course, there is a very intimate relation between molecular grouping and the optical features observed. We touch here on the boundaries of our present physical knowledge. That light consists in the vibrations of a subtile medium or *ether*, that self-luminous bodies, including phosphorescent bodies, which are for the time being self-luminous, are in a state of molecular agitation which they are capable of communicating to the ether, that consequently in the phenomenon of absorption molecular disturbance is excited in bodies at the expense of etherial vibrations—all this is so well established as to leave no reasonable room for doubt. But what may be the precise nature of the molecular vibrations, what may be the mode of connexion by which the vibrations of the ether agitate the molecules, or the molecules in their turn are able to agitate the ether, what may be the cause of the diminished velocity of propagation in refracting media, what may be the mechanical cause of the difference of the velocity of propagation of right and left-handed circularly polarized light in media like sirup of sugar, which is manifested by a rotation of the plane of polarization of plane-polarized light, still more what may be the nature of the action of magnetism in respect of the propagation of light through bodies—all these are questions concerning the true answers to which we can affirm nothing, though plausible conjectures may in many cases be framed.

We have seen how searching are the phenomena of light with respect to the molecular constitution of bodies, although I have said nothing relating to the information they afford concerning the molecular structure of crystals. The molecular groupings on which I have chiefly dwelt as illustrated by the phenomena of light are mostly of a complex character, chiefly belonging to organic substances, and of a kind which could not exist at a high temperature. The subject which I propose next to bring before you relates almost exclusively to bodies in a state of incandescence, so that organic combinations are excluded. If on the one hand the field of research is thereby limited, on the other the nature of the phenomena connects them with other branches of science, rendering the subject of more general interest.

It has long been known that salts of particular metals, such as those of the alkalies and alkaline earths, when introduced into a feebly luminous flame, such as that of a spirit lamp, cause a coloration depending on the base of the salt. Thus salts of potash give a violet, of soda a yellow, of lithia a red, of baryta a green, of strontia a red, of lime a brick red. The colours thus produced have to a certain extent been used by mineralogists in discriminating between different minerals by the aid of the blowpipe.

But just as the prismatic analysis of the colours due to absorption reveals characters of the absorbing body which are often highly distinctive, but which would escape detection so long as we merely observed the absorption colour with the naked eye, so here a prismatic analysis of the coloured flames reveals immensely more than can be perceived merely by looking at the colour. As long ago as 1834, the late Mr Fox Talbot showed that the red due to a salt of strontia and the red due to a salt of lithia can be at once distinguished by the prism, which in the case of lithia shows a narrow well-marked line in the red, not far from the line C, while the spectrum of a strontium flame wants this line, and is of a more complicated character. Several years before, both Sir John Herschel and Mr Talbot had drawn attention to the characteristic lines produced on introducing the salts of certain bases into flames, and had pointed out how small a quantity of a substance suffices to produce the effect, though it is true that Talbot entered into some erroneous conjectures as to the origin of the bright line D.

It is remarkable for how long chemists neglected the precious means of discrimination lying at their very hands in the use of the prism—a striking example of how much may be lost by a too exclusive devotion to one branch of science to the neglect of others. Notwithstanding that W. A. Miller had published maps of the spectra of flames coloured by the alkalies and alkaline earths, it was not till Bunsen and Kirchhoff published their celebrated researches that spectral analysis came into regular use with chemists.

Bunsen and Kirchhoff engaged in a methodical chemico-optical examination of the spectra of salts introduced into an almost non-luminous flame, for which they used the flame of a mixture of air and coal gas, burnt in a Bunsen's burner, taking the greatest care to purify the substances used, and examining separately the spectra given by the same base combined with a variety of acids. They found that the spectra depended on the metal of the salt, and not on the acid radical; the acids which were sparingly volatile merely showing the spectra more feebly. The spectra showed bright lines or narrow bands characteristic of the metals; the heavy metals as a rule showed no spectra when examined in this way. The order of importance of the various bands in the spectrum of the same metallic salt as tests of the presence of the metal was determined, and thus it became easy to detect even small quantities of these metals present in a mixture.

These investigations were almost immediately rewarded by the discovery of two new elements, Rubidium and Caesium, which were traced by the appearance of certain bright lines in the spectrum of a Bunsen flame when a variety of specimens of substances from different localities, including waters from mineral springs, were introduced into it. They proved to be the metals of two oxides of the group of alkalies, and were named after the colour of their most distinctive bands. Nor was this all. The facility of the test, which though indicated long before had not been put in practice, enabled Bunsen and Kirchhoff to show that lithia, which previously had been regarded as a rare substance, was on the contrary very widely distributed, though usually present in small proportion, and led to the discovery of sources and means of separation of the alkali by which it can be obtained at a comparatively cheap rate.

The method of spectrum analysis, carried out as above indicated, or modified by the employment of a succession of electric discharges instead of a Bunsen flame, has led in the hands of others to the discovery of three more new metals, namely Thallium independently by Crookes and Lamy, Indium by Reich and Richter, and Gallium by Le Coq de Boisbaudran.

In their original paper, Bunsen and Kirchhoff contented themselves with establishing the fact that different salts of the same metal when introduced into a Bunsen flame gave the same spectrum, which could therefore be used as a test of the presence of the metal; they did not commit themselves to any theory as to what the particular vapour present in the flame might be which produced for each metal its characteristic spectrum. That it was a vapour of some kind, follows both from the circumstances of the experiment, and from the consideration that it is only in the state of vapour that substances exhibit such narrow absorption bands as are actually produced by the flames, or as would correspond to the bright lines in the spectrum of the light they emit. It might be supposed that as different salts of the same metallic oxide yield solutions which as a rule exhibit similar modes of absorption, so different salts when volatilized in a flame might yield vapours consisting of the salts themselves, and yet having a spectrum in common. Or it might be supposed that the identity of the spectra was evidence that the salts were decomposed in the flame, and that the glowing vapour which yielded the spectrum common to them all was that of the metal itself.

But a different conclusion resulted from the observations of Alexander von Mitcherlich on the spectra of the chlorides, bromides, iodides and fluorides of the alkaline earths. When a bead of a chloride for instance is introduced into a Bunsen flame, in the manner followed by Bunsen and Kirchhoff, a spectrum with the bands of the salts in general of the same base is obtained. But as in this mode of observation a minute quantity of the volatilized chloride is present in an atmosphere in which there is plenty of oxygen and vapour of water, it may very well be that the chloride is decomposed, with the formation of an oxide and hydrochloric acid. To prevent this von Mitcherlich used a solution of the chloride to which a comparatively large quantity of sal ammoniac, which itself gives no spectrum, was added. In this way the volatilized chloride was present in an atmosphere abounding in hydrochloric acid (from the temporary dissociation of the sal ammoniac) and was accordingly maintained as such, and now the spectrum showed bands indeed, agreeing so far with the spectrum obtained in the former manner, but the actual bands were quite different. Accordingly we must infer that in this case the glowing vapour was the chloride, but in the former method the oxide.

The other haloid compounds behaved in a similar manner, showing spectra differing from each other, and from that of the oxide. Moreover there was a remarkable similarity of character between the spectra of the chloride, bromide and iodide of the same metal; a group for instance of bright lines in the chloride having corresponding to it in general arrangement, but differing a little in position, a group in the bromide, and the latter again having corresponding to it a group in the iodide. Moreover the order of the change corresponded to the chemical order, the bands of the bromide being intermediate between those of the chloride and those of the iodide, just as in its chemical relations bromine is intermediate between chlorine and iodine. We may infer that the vibrating molecular systems which disturbed the ether, and were thus the source of the light, were the compounds, the chloride, bromide and iodide, of the metal; and we have here another example showing how closely the optical phenomena presented by bodies touch their molecular structure.

The same conclusion results from a comparison of the spectra of ordinary flames into which certain salts are introduced with that obtained from the more powerful incandescence produced by electricity. When an electric discharge is passed between electrodes formed of the metals themselves, or electrodes wet with solutions of their salts, spectra are obtained which in some cases are quite different from what are shown by flames. In the case of the alkalies, the same lines are seen as in flames, with the addition of others of similar character. But in the case of the alkaline earths, in lieu of the bands seen with flames, narrow bright lines are seen, at least with the jar discharge, occupying different positions from the bands. The augmentation in the number of lines brought out in the first case, that of the alkalies, is explicable by the greater intensity of the incandescence, and we infer accordingly that the lines seen in the flame spectra are those due to the metals themselves, of which the oxides are the alkalies, while the difference in the spectra in the second case indicates that the bands seen in the flame spectra of the alkaline earths are referable to some compound of the metals, and accordingly, as the burning takes place in air, to the oxides, that is, to the alkaline earths themselves.

The methods by which the chemist judges of the nature of the bodies with which he has to deal are restricted to such substances as he can get access to, so as to subject them to his manipulation, but light may be examined at any distance from its source which is not so great as to render it too feeble for observation; and when the source is one of great intensity and magnitude, that distance may be enormously great. Hence such information as may be obtained from the character of the light respecting the chemical nature of the source from which it comes is available for the examination of bodies at a distance. This leads me into a field of great interest, for a reason which I will not anticipate by mentioning at present.

The celebrated Fraunhofer, after whom the fixed lines of the solar spectrum have been named, applied the admirable prisms and other optical apparatus of his own manufacture to the examination of other sources of light. He found that while the same system of dark lines is seen in the light of the sun, whether coming directly, or reflected from the clouds or from the moon or planets, so far as the feebler light in the latter case admitted of a comparison, the fixed stars showed systems of dark lines differing from the solar system and from one another, though having some of the lines common to two or more of the systems. This afforded confirmation from an unexpected quarter of what was already well established on astronomical grounds, that the moon and planets shine by light derived from the sun and reflected by those bodies, whereas the stars like our sun are independent and self-luminous.

Fraunhofer found further that artificial sources of light in some cases show bright lines in their spectrum. The discharge of an electric machine in action showed a whole system of bright lines. He further observed that the light of a candle shows a bright line in the yellow *exactly coinciding* in position with the dark line D of the solar spectrum, and like it double. Fraunhofer did not advance any hypothesis to account for this coincidence, which is too remarkable to be attributable to a casual similarity of position so close as to appear to be an identity.

This conclusion long remained an isolated fact without explanation. But in the year 1849 Foucault made a remarkable step in advance. He was engaged in comparing the brightness of the electric light with that of the sun; and in the course of his researches he had occasion to send a reflected beam of solar light across the arc which connected the carbon poles, using a lens to form an image of the sun at the place of the arc. He noticed that in the solar beam which had traversed the arc the double dark line D appeared darker than usual, while when the sun's light was intercepted, and the only light was that of the arc, a bright line was seen in the same place. When the sun's image was arranged to cover a part only of the arc, so that the light passing through one part of the slit was derived from the arc alone, and that passing through the rest consisted of the light from the arc together with the solar light which had passed through it, the strip of the spectrum corresponding to the first part of the slit showed the bright D, while the remainder of the spectrum, corresponding to the compound light, showed a dark D which was *an exact prolongation* of the former, demonstrating the complete coincidence of position of the dark and bright D. Moreover the dark D could be obtained independently altogether of solar light by sending through the arc by reflection the light from one of the incandescent poles. The glowing poles alone give a continuous spectrum, and the spectrum of the compound light obtained as above described showed a dark line D just like the solar spectrum. This shows that the electric arc, which emits light of the definite refrangibilities of the two components of the Fraunhofer line D, acts also as an absorbing medium which absorbs light of the same definite degrees of refrangibility.

I was very much struck with this observation when Foucault mentioned it to me a few years later in conversation. It seemed to me that a dynamical illustration of how a medium could act both by emission and absorption for light of a definite refrangibility was not far to seek. Imagine a series of stretched wires like pianoforte wires all tuned to the same note. The series if agitated, suppose by being struck, would give out that note, which on the other hand it would be capable of taking up if sounded in air. To carry out the analogy, we have only to suppose a portion of the molecules constituting the vapour of the arc to be endowed with a capacity of vibrating in a definite manner, that is, according to a definite time of vibration.

But what were these molecules? It is well known that the bright D in flames is specially characteristic of compounds of sodium, though from its very general occurrence some had doubted whether it were not really due to something else. But in what condition must we suppose the sodium in the arc to be? The compounds of sodium, such as common salt, carbonate of soda, &c., are colourless; and it would be contrary to the analogy of what we know as to the relation of gases and vapours to their liquid or solutions to suppose that a gas which does exercise absorption should be merely the vapour of a heated solid which does not. On this ground it seemed to me that the substance which exercised the selective absorption in Foucault's experiment must be free sodium. This might conceivably be set free from its compounds in the intense actions which go on in the sun or in the electric arc; but I had not thought that a body of such powerful affinities would be set free in the gentle flame of a spirit lamp, nor perceived that the fact of that flame's emitting light of the definite refrangibility of D entails *of necessity* that it should absorb light of that same refrangibility.

To enable you the better to follow what I have now to bring before you, it will be well to enter on a brief digression.

Conceive a closed opaque envelope of any kind to be uniformly and permanently heated. If a thermometer be introduced into the envelope, the mercury will begin to rise, but soon the rising will become slow, and presently the mercury will have risen to a height which no longer alters, and which depends on the temperature of the envelope.

Now how is it that the thermometer receives the heat which causes the mercury to rise? Partly no doubt it is through contact with the heated air inside the envelope. The air in contact with the bulb gets cooled and descends, giving place to warmer air which in its turn warms the thermometer; while the air which was cooled by contact with the thermometer gets warmed by contact with the envelope, and so becomes ready to warm the thermometer when it gets to it. But this is far from accounting for the whole effect, for the rise of temperature of the thermometer takes place equally when the envelope is exhausted as completely as possible of air, though not, it is true, so rapidly. Consequently a large part of the rise of temperature must be attributed to the radiation of heat from the walls of the envelope, across the intervening space, to the bulb of the thermometer.

But what takes place when the thermometer has come to its permanent condition? Are we to suppose that the radiation then ceases? We have the strongest reason for believing that the radiation of heat is perfectly analogous to the radiation of light, and consists in the communication of a disturbance from the ponderable molecules to the ether. But the envelope is now just in the same state as before, that is, as it was when the mercury of the thermometer was still rising, and the ether is there as before to receive the motion communicated to it. The molecules of the envelope cannot prophesy what is ultimately to become of the motion they may communicate to the ether, and regulate the communication accordingly. We must suppose the radiated disturbance to proceed from the walls to the bulb just in the same manner whether the bulb be hot or cold. Falling on the bulb, it will be in part absorbed by it in the same manner in the two cases. How is it then that the temperature of the bulb does not go on rising? Why, just as the envelope radiates towards the bulb, the bulb radiates towards the envelope. The amount radiated will depend on the temperature, the nature of the bulb being supposed given; the higher the temperature the more copious will be the radiation. If the bulb gains in temperature by the heat that it receives by radiation from the walls, it loses in temperature by the heat which it radiates towards the walls. The actual change is merely the balance between the two; and thus we are led to infer that even when everything has come to its permanent state, and there is apparently perfect rest within the envelope, there is still a radiation and absorption of heat going on; the bulb radiates heat towards the envelope, and receives heat from it by its radiation; and as the thermometer remains steady, we infer that there is an exact balance between the loss of heat on part of the bulb by radiation and its gain of heat by absorption. The steady height at which the thermometer stands is found to be the same wherever the thermometer may be placed within the enclosure, and whatever may be the nature of the covering, if any, of the bulb, and to be that marking the temperature of the envelope as determined by other means.

But while the nature of the covering of the bulb does not affect the ultimate height at which the thermometer stands, it affects most materially the rate at which that ultimate height is approached. If the bulb be coated with lamp black, the thermometer rises more promptly, if with silver more sluggishly. We infer from this, what we know by independent means, that silver absorbs radiant heat less readily than glass, and glass again than lamp black. And as in each case there is a perfect balance between the heat absorbed and the heat radiated, we infer that the absorbing power of a substance at any temperature corresponds to its emitting power at the same temperature.

The theory above mentioned of the mode by which the equalization of temperature is effected, and of the action that still goes on when the temperature has become uniform, is due to Prevost, and is called Prevost's theory of exchanges.

Hitherto we have spoken only of the total quantity of heat radiated or absorbed, without regard to quality. But we know that radiant heat like light consists of kinds differing from one another in refrangibility; and just as a body may be transparent for light of one kind and opaque for light of another, comparatively speaking at least, so a body may be comparatively transparent for heat of one kind and opaque for heat of another. Now a very important extension of Prevost's law of exchanges was made by Professor Balfour Stewart, who instituted a series of experiments on the quality of the heat radiated by rock salt and other bodies, as tested by the capacity of the heat to pass or not to pass through plates of the same substance as that which emitted the heat. He showed for instance that the heat radiated by rock salt is in good measure absorbed by a plate of rock salt, a body which so freely transmits most kinds of heat that it is sometimes regarded as perfectly diathermanous. All his results he showed to be in accordance with an extension of Prevost's theory of exchanges, according to which the balance between radiation and absorption holds good, not merely for heat taken as a whole, that is, for all kinds taken together, but for each particular kind of heat in particular.

The transition is easy from heat to light; in fact we have the strongest reasons for believing that physically considered there is no greater difference between radiant heat and light than there is between light of one colour and light of another. Conclusions of a similar nature concerning light accordingly naturally presented themselves to the mind of Professor Stewart; and he was on the point of publishing some experiments in this direction when he was anticipated in the extension of Prevost's theory to light of each kind in particular, and in the more complete experimental establishment of the legitimacy of that extension which the facility of observing light renders possible, by Professor Kirchhoff, who had been led to it quite independently of what Professor Stewart had done in heat, and who deduced from it most important consequences.

In the year 1860 Kirchhoff, who was engaged along with Bunsen in a very important series of researches to which I have already had occasion to allude, discovered that when light from a source at a sufficiently high temperature was passed through a flame which showed the bright line D, in consequence, as he and Bunsen had now conclusively proved, of the presence of sodium, in some state, a dark line D was artificially produced by absorption of the light which was passed through the flame. He was not aware at the time that the same thing had been discovered many years before by Foucault in the particular case of the electric arc. He however was led to generalize the phenomenon, and to affirm that flames which show bright lines or bands in their spectrum must for that very reason act as absorbing media, absorbing light of the same refrangibilities sent through them. Whether in the spectrum of the compound light, consisting partly of the light emitted by the flame, partly of the light sent through it, the line shall appear as bright on a dark ground or as dark on a bright ground, depends on the relative temperatures of the flame and of the source from whence the light sent through the flame proceeds. If the flame be the hotter of the two, and we contrast the region of the line with the parts of the spectrum immediately on both sides of it, there is more gain of light by emission from the flame than loss of light by absorption of light sent through it, and the line therefore appears as bright on a less bright ground. If on the other hand the source is the hotter, and is opaque, at least in the region of the line and its neighbourhood, so as to give the full radiation due to its temperature, and the light comes from it direct, not weakened by reflection or otherwise, then the gain of light by emission from the flame is more than compensated by the loss by absorption of light coming from the source, and the line is seen as dark on a brighter ground.

We have hitherto considered two independent sources of light, one the flame, the other the source of light which sends its light through the flame. But the different parts of a flame are not all at the same temperature; the outer mantle is cooler than an envelope inside it which is the chief seat of the combustion. Accordingly in certain cases the outer envelope takes the place of the flame in the above explanation, the inner shell that of the body of higher temperature which sends its light through it. Thus when a Bunsen flame is richly fed with chloride of sodium, and the spectroscope is one of high dispersion, each component of the D line is seen considerably widened, and in the middle of each is seen a hair-like dark line, giving a pair of dark lines just as in the solar spectrum.

LECTURE III.

Inferences deduced from a study of the dark lines in the solar spectrum as to the presence of certain chemical elements in the sun, and as to the condition of that body—Spectra of the stars, including the examination of the ultra-violet region—Resulting classification of the stars—Nebulae—Character of their spectra, and inferences thence derived as to their constitution—Examination of the star which burst out in the Northern Crown in 1866*—Comets, and character of their Light—Theory, in some respects new, of these bodies.*

OF the dark lines of the solar spectrum, some are attributed to absorption of light in the atmosphere of the earth, and accordingly become much more conspicuous when the sun is very low. These however appear to form a small minority, and to lie mostly in the less refrangible portion of the spectrum. The others must be attributed to the quality of the light as it comes to us from the sun. It is only through the absorption of light by vapours that we are able to imitate the solar spectrum in this respect, that we get a spectrum interrupted by numerous dark lines or narrow bands. This consideration points to absorption in the sun's atmosphere as the probable source of the lines; and the relation between absorption and emission brought out so clearly by Kirchhoff leads us to seek among vapours which show bright lines by emission for coincidences of position between such bright lines and dark lines of the solar spectrum.

In the discharge of electricity, whether between the poles of a powerful battery, or more conveniently between electrodes connected with the secondary terminals of an induction coil, we have a source of temperature far surpassing what can be obtained merely from flames, and which is competent to volatilize even such refractory substances as iron and platinum. It might be that in this way we should obtain such evidence of coincidence of position between bright lines artificially produced and the natural dark lines of the solar spectrum as to lead us to the conclusion that the substance volatilized in the discharge was also present in the atmosphere of the sun as an absorbing vapour.

But the number of dark lines in the solar spectrum is extremely great, and the same is true of those obtainable from the electric discharge passed between different substances. A coincidence or apparent coincidence of position between a bright and a dark line *might* therefore be merely casual, and not indicate any real physical connexion. I say "or apparent coincidence," because our means of judging of the exactness of what appears to be a coincidence are finite. The greater the precision with which we can judge of coincidences of position, the greater is the probability that an apparent coincidence is a real one, and indicative of a physical connexion. And the probability would be enormously increased if the substance under examination should be found to show not merely one but a series of bright lines having, apparently at least, coincident with them a corresponding series of dark lines in the solar spectrum.

The sharpness with which we can compare the coincidence of position of a bright artificial line and a dark solar line, whether seen simultaneously or successively, in the same spectrum, depends on the purity and angular extent of the spectrum. For the sake of subjecting the exactitude of any apparent coincidences to a very searching scrutiny, Kirchhoff was led to construct a most elaborate and detailed map of the solar spectrum, at least of the brighter part of it in the first instance, surpassing anything of the kind that had been done before. The map contains for comparison the places of the bright lines seen when an electric discharge is passed between electrodes of various metals.

In several cases the coincidences were so striking that there could be no doubt of the reality of the connexion, and consequently of the presence of the substance in question in the atmosphere of the sun.

Let us take for example the particular case of iron. The bright lines in the spark spectrum of iron are very numerous, and yet these lines are sure to have corresponding to them in position dark lines in the spectrum of the sun; and not only so but in general the lines which are strong in iron have strong dark lines agreeing with them in position in the spectrum of the sun.

Let us pause for a moment to reflect on the conceptions which these results open out to us of the state of incandescence of the sun. That it is intensely hot, must have been inferred from the earliest times from the brilliancy of its light and the warmth which we experience when it shines on us. But this does not furnish us with any estimation of *how* hot it is, or even enable us to say what effects at least its temperature must be capable of producing. The observations I have last mentioned show that even its outer and somewhat cooler portions must still be above the *boiling-point* of iron, hot enough, that is, to maintain iron in the condition of a permanent gas. And if even the outer portions are at such an enormous temperature, what must be the condition of the interior?

It is true that some physicists, some even of great eminence, have speculated on the body of the sun being of a comparatively moderate temperature, and have supposed that the intense luminosity was confined to an envelope, the so-called photosphere, forming the lower portion of the sun's atmosphere; though it must be noted that the term "photosphere" is employed also by those who do not share the theory of a comparatively cool nucleus. This strange theory, so contrary to all that we know as to the behaviour of a body within a heated envelope, was probably devised to account for the phenomena then known as to the behaviour of the dark spots on the sun's surface. But besides the inherent improbability of any such hypothesis, later research has revealed phenomena of the spots which ill accord with the idea of a comparatively dark solid nucleus. And we shall see presently that we have evidence of tremendous actions going on in the sun, of a piece with the enormous temperature which even from the observations already mentioned we must attribute to it.

Besides sodium and iron, the first researches of Kirchhoff indicated magnesium, calcium, chromium, nickel, perhaps cobalt, and probably barium, copper and zinc, as present in the atmosphere of the sun. Later researches conducted, partly by further carrying out the same method, partly by following the guidance of new theoretical views, have considerably added to this list.

But there is one element, chemically indeed analogous to a metal, but very different from the metals as regards the physical condition in which these substances commonly present themselves, the indication of the presence of which in the sun must particularly be mentioned; I allude to hydrogen. When an induction discharge is passed through a Geissler's spectral tube containing rarefied hydrogen, and the spectrum of the bright discharge in the capillary part is examined, it is seen to consist mainly of three bright lines or narrow bands, for they are not mere lines, with a fourth further on in the violet. Now all four coincide exactly in position with four conspicuous dark lines in the solar spectrum, of which the first two were among the standard lines selected by Fraunhofer, namely, C in the red and F in the blue. We infer therefore that hydrogen is present as an absorbing gas in the sun's atmosphere; and what confirms the correctness of the conclusion is that in the spectra of others of the heavenly bodies the lines C and F are usually seen to go together, that is, to be present or absent together.

It may be said, How can we suppose that hydrogen thus exercises an absorbing influence, seeing that when examined on earth, even in considerable lengths, it appears perfectly transparent?

Of course the distance that the sun's rays must travel through hydrogen in the sun's atmosphere if hydrogen be there must be enormous compared with anything that we can imitate by experiments on earth; and it may be that very great lengths of transit are necessary to bring out the distinctive absorption due to hydrogen. Yet considering how small a thickness of the gas suffices to bring out in great intensity the bright lines of the gas, and how small a thickness of vapour of sodium or lithium suffices to show the selective absorption of the vapours of those metals when a salt of one of them is introduced into a Bunsen flame, this explanation seems open to a good deal of doubt; though on the other hand the fact that a selective absorption has recently been shown to take place in oxygen when great thicknesses are looked through, though in moderate thicknesses the gas appears to be perfectly transparent, shows that the explanation is far from impossible. It is to be noticed however that the equality between emission and absorption for each kind of light in particular can only be affirmed as of necessity when the gas is of the same temperature as the opaque body which emits the light that passes through it. Should the absorbing power of a gas for light of a given kind alter with the temperature of the gas, as may well be the case when we have to deal with such enormous differences of temperature as that obtained by an electrical discharge and ordinary temperatures, the result mentioned might very well follow; the gas might be transparent at the low temperature, and yet might exercise a selective absorption at the higher. Or again it is conceivable that the molecule of hydrogen may be temporarily split up by the discharge of an induction coil, or permanently so by the enormous temperatures which prevail in the sun, and that it is the product of dissociation which exercises the selective absorption. In any of these ways the difficulty that has been mentioned may admit of explanation, though we are not able at present to say what the true explanation is.

I mentioned in my last lecture that Fraunhofer observed that different fixed stars show different systems of dark lines in their spectra. Now that we are able, to a considerable extent at least, to connect these dark lines with the presence of particular elements, we are enabled by spectroscopic observation to gain some information respecting the chemical constitution of these bodies, situated though they are at distances from us of which we find it difficult to get an adequate conception. No one has distinguished himself by working in this field more than our own countryman Dr Huggins, the firstfruits of whose labours, at that time in conjunction with the late Dr W. A. Miller, are published in the Philosophical Transactions for 1864. Dr Huggins has since continued his labours in this field, and after encountering many difficulties has successfully applied photography to the delineation of the spectra of the brighter stars in the more refrangible part of the visible spectrum, and in the ultra-violet region where eye observation fails altogether. The results are published in the Philosophical Transactions for 1880.

The general character of the results of the whole investigation is very striking. While as was previously known the stars agree with one another, and with our sun, in the character of giving a spectrum interrupted by dark lines, these more extended observations—more extended as embracing a region inaccessible to the eye used directly—point towards a classification of stars on the whole in approximate order of sequence. The white stars, which as he had previously found show very strongly the dark lines C and F, in the visible spectrum, which are attributed to hydrogen, showed a whole series of dark bands, as many as 12 in α Lyrae, which were arranged in a very regular manner, decreasing in width and distance apart in going in the direction of increasing refrangibility. They were the same in the different stars, except that in some a few of the more refrangible lines could not be seen in the photographs. They gave a decided appearance of law, as if they belonged to one another, and to the visible lines C and F. Moreover the first five of them, of which the first two belong to the still visible but weaker part of the visual spectrum, have been identified in position with the bands of incandescent hydrogen. There appears to be very little doubt that they belong to hydrogen, which in these stars shows itself more strongly than in our own sun. In these stars the other dark lines, mostly referable to metals, are fine and inconspicuous. On the other hand the bright but reddish star Arcturus shows a complicated spectrum more nearly resembling in character that of the sun, but in some respects differing from the spectra of the white stars even more than does that of our sun. And Secchi found that the spectra of the decidedly red stars, which are all small, showed shaded bands analogous in character to some of the spectra artificially produced by the electric discharge, the origin of which, that is, whether they belong to elements or compound bodies, is still, in some cases at least, a matter of dispute.

It is noteworthy that some of the stars show among their more conspicuous lines series answering to those of elements which are by no means common on the earth, and which appear to be either absent or not prominent in the sun. Thus in the two stars the spectrum of which is figured by Huggins and Miller in the Philosophical Transactions for 1864, namely Aldebaran and α Orionis, there are several coincidences in both cases with each of the elements bismuth and tellurium.

The general result tends to establish a similarity of plan, combined with individual differences, between the different fixed stars, and between them and our own sun.

We can hardly avoid surmising that these distant suns may, like our own sun, be accompanied by planets circulating around them, and that these planets again, or such of them as may be habitable, are like our own earth tenanted by living beings, it may be by rational beings of some kind.

I come now to another class of heavenly bodies which have long excited the interest and curiosity of astronomers, and on the constitution of which spectroscopic research has of recent years cast a new and unexpected light; I allude to the nebulae.

When the heavens are examined with a good telescope, here and there among the stars are seen bodies differing from the stars in the circumstance that their light is not concentrated into a point, but is more or less diffused, and subtends, as seen through the telescope, a very appreciable angle. If seen they could not ordinarily be recognised with the naked eye, but the hazy appearance of the great nebula in Orion may at this time of year be noticed any clear evening even with the naked eye. Among them are several which show a round outline, from whence they have received the name of planetary nebulae.

Now what are these nebulae? Are they luminous bodies of a really continuous structure, or are they merely clusters of stars, either so minute, and comparatively speaking near together, or so distant, and apparently near together, that they cannot be distinguished individually? We are familiar with the gleam of light which constitutes the milky way. We cannot with the naked eye resolve it into stars, yet with the telescope it is so resolved; may it not be that even the nebulae are merely clusters of stars which our telescopes are unable to resolve?

Sir William Herschel was of this opinion, and supposed that our sun and the brighter stars which we see were merely the portions comparatively near the centre, while the milky way formed the distant outlying portions, of a vast flattened cluster of stars, which at an enormously distant point in the universe might as a whole appear as a nebula, like what the planetary nebulae appear to us; and conversely, that the planetary nebulae were groups of a somewhat similar constitution, which cannot be resolved, and gigantic as the linear magnitude of the systems may be subtend as a whole merely a small angle, in consequence of the almost inconceivably great distance at which we see them. Laplace on the other hand supposed that the nebulae were really diffuse luminous matter in process of forming suns by the condensation due to the mutual gravitation of their parts. In many cases the nebulae exhibit stellar points in an approximately central position; too frequently to allow us to suppose that they are merely stars which happen to be in the same direction as the nebulae, but which may really be immensely nearer to us or further from us than the actual nebulae. According to Laplace's views, these stellar points would be stars in process of formation.

As telescopes were improved in power and sharpness of definition, our ability to resolve clusters of close stars was naturally increased. In particular, when the magnificent six-foot reflector constructed by the late Earl of Rosse was applied to the scrutiny of the heavens, what had formerly appeared as patches of light were in some cases resolved into clusters of stars, while in other cases they still resisted resolution. I recollect long ago at a meeting of the British Association hearing the late Dr Robinson, in the course of some remarks on the results obtained through Lord Rosse's telescope, state that the balance of probability seemed to him now to lie in favour of the supposition that if some nebulae still appeared to be continuous in constitution, though showing the structure, frequently of a spiral or wisp-like character, which that wonderful instrument revealed, it was only because in spite of the great advances which had been made our instruments were still of finite resolving power. In this unsettled state the subject remained for many years, until an unexpected discovery set the question at rest.

Immediately following the paper by Dr Huggins and Professor Miller in the Philosophical Transactions for 1864 to which I have already alluded, is a paper by Dr Huggins on the spectra of the nebulae. In this he relates how on turning the Royal Society's telescope which was entrusted to him to one of the planetary nebulae, he was surprised by finding its spectrum to consist of three isolated bright lines, of which the first coincided in position with a line of nitrogen, the third with the line F of hydrogen, while the intermediate line did not agree with that of any known element, though it lay near a line of barium. A number of nebulae to which the instrument was directed showed the very same spectrum, except that the more refrangible and fainter of these lines was frequently invisible, while on the other hand in one case a fourth, more refrangible line was faintly seen, which coincided with the line of hydrogen near G. These nebulae frequently contained stellar points, corresponding to which was a narrow continuous spectrum, interrupted probably by dark lines which there was hardly light enough to see.

Now as it is only when matter is in the state of gas or vapour that when rendered glowing it gives out a spectrum with isolated bright lines, we have a right to conclude that these nebulae, making abstraction of the stellar points, consist of glowing gas.

But what is the gas? This is a question not so easily answered. The fact that the most refrangible of the three lines in the nebulae coincides in position with the line F, which is the second of the conspicuous lines of hydrogen, points to hydrogen as probably one of the gases present. This conclusion seems to be rendered almost certain by the circumstance that in one of the nebulae Dr Huggins was able to make out, in addition to the three bright lines characteristic of the nebulae in general, a fourth bright line considerably more refrangible, which coincided with the third conspicuous line of hydrogen, that near G, as was also seen later in the nebula of Orion.

The fact that the bright line C in the red, which in a Geissler's tube is commonly the most conspicuous of the hydrogen lines, is absent from the spectra of the nebulae, might seem to throw a difficulty in the way of this conclusion. But Plücker and Hittorff found that when the gas is very highly attenuated the line C disappears; besides which it is to be borne in mind that when the intensity of light is greatly reduced, the light of lower refrangibility ceases to be perceived before that of higher.

There remain two of the nebular lines to account for. The least refrangible coincides with a line of nitrogen, or rather with one of its components, for the nitrogen line was generally seen double, the nebular line coinciding with the less refrangible component, while the line in the nebula always appeared single. Dr Huggins accordingly left it doubtful whether the first of the nebular lines ought properly to be referred to nitrogen, though later research seems to favour the supposition that it ought. The second line however at any rate still remains unaccounted for. It may possibly indicate some form of matter more elementary than any we know on earth.

There seems no *a priori* improbability in such a supposition so great as to lead us at once to reject it. Chemists have long speculated on the so-called elements, or many of them, being merely very stable compounds of elements of a higher order, or even perhaps of a single kind of matter; and the combination of observations of the sun's surface with experiments in the laboratory have been thought by some to be favourable to such a view.

Another example of the information obtained by recent methods of research as to what is going on in distant parts of the universe is afforded by an event which took place in the year 1866.

On more than one occasion it is recorded in history that a new star burst forth in the heavens, and after remaining brilliant for a considerable time gradually faded away. One such star appeared in the time of the Greek astronomer Hipparchus. Another was seen in the time of Tycho Brahe. The latter was as bright as one of the principal planets, and remained visible for many months before it faded away, and that so completely that now no star appears in its place.

Now a somewhat similar phenomenon was seen in 1866, though on a much smaller scale both as to magnitude and duration. A star appeared in the constellation of the Northern Crown which observers who had been so in the habit of watching the heavens that they had as I may say got the stars by heart recognised as new. Of course the discovery was immediately published, and astronomers directed their instruments to the new comer. It was none too soon, for the brightness of the star waned rapidly, so that even one or two days' delay made a difference. At the brightest, the star did not near equal the one which appeared in the time of Tycho Brahe; but not to mention the improvement in telescopes, astronomers are now in possession of an instrument unthought of in his days; I allude to a star spectroscope. When the new star was examined by the spectroscope, the spectrum was found to be of a composite character, resembling the ordinary spectrum of a star with a bright-line spectrum superposed on it. This indicated that a good part of the light coming from the star was due to incandescent gas; and Dr Huggins mentions that as the star faded, the continuous spectrum became relatively feebler than the spectrum which consisted of bright lines. In the bright-line spectrum the indications of hydrogen were as usual conspicuous.

It does not seem probable that such a unique event in the history of a star is merely an exaggeration of the regularly recurring phenomena of a change of brightness in the so-called variable stars, more especially as spectroscopic examination has not hitherto revealed the existence of a bright-line spectrum in the light of the variable stars. It would rather seem to point to some tremendous conflagration, whether due to the collision of two bodies in the interstellar spaces, or to some other cause. The distance to which the glowing gas extended must have been enormous to have subtended a sensible angle at the earth, giving in the telescope the appearance of a misty star. What may have been the date of the actual occurrence which we witnessed in 1866 we do not know. It must have been several years earlier, as light would take years to travel from the star to us.

In contemplating so gigantic a catastrophe, our thoughts can hardly fail to turn to what we are led to expect as the ultimate fate of our earth.

There is another strange class of heavenly bodies, belonging to our own solar system, in the elucidation of the nature of which the properties of light have been of much avail; I allude to comets. Briefly to refer to what will be found in elementary books, I may say that they move in orbits which are elliptic and highly eccentric, with the sun in one focus of the ellipse, or commonly in orbits which are sensibly parabolic for that part of them in traversing which the comet can alone be seen. It is said that some comets have been ascertained to move in hyperbolic orbits, in which case they must have been temporary visitants to our system, or at least on leaving the sun have gone off into space. In either case the comet is comparatively near the sun for a limited part only of its orbit. In approaching the sun from a distance, it at first appears like a misty star, but as it gets nearer a tail appears, which rapidly increases in length as the comet approaches its perihelion, or point of its orbit nearest to the sun, after passing which the tail is commonly even better developed than at an equal distance before perihelion. It is remarkable that at least with rare exceptions the tail is turned away from the sun, whether the comet is approaching perihelion or receding from it. It is also curved, the convexity being on the side towards which the comet is moving.

Now what notion can we form of the nature of that strange appendage, the tail, darted forth from the comet, if darted forth it be, with a velocity sometimes so enormous, whisked round, if whisked round it be, as the comet passes perihelion, with a velocity in a direction perpendicular to the radius vector drawn to the sun which in the outer portions of the tail must immensely exceed that of the comet itself?

To evade these difficulties, some have broached theories according to which the tail would not consist of matter derived from the comet, that is, from its nucleus, but would arise from some change of condition, or difference in the mode of illumination, of matter there already. But among astronomers, who were familiar with the appearances of the heads of comets as seen in good telescopes, the opinion I think was generally entertained that a portion at least of the envelope and tail consisted of matter ejected from the nucleus.

Let us now see what evidence can be brought to bear on the question by an examination of the light of the comet.

If the tail were self-luminous, its light ought to show no polarization. If it were due, in part at least, to the reflection of light from the sun falling on dust or condensed vapour, it ought to show more or less distinct signs of polarization. The fact is that the light of the tail shows very decided traces of polarization, as has long been known.

Of recent years the light of comets has been examined spectroscopically, by Dr Huggins and others. The result is very remarkable. The nucleus and a small portion of the root of the tail are found to show a spectrum with bright bands, which are mainly the same in the greater number of comets, though some show peculiarities of their own, and which curiously enough agree with the bright bands shown in the slightly luminous flames of hydrocarbons, such for example as the blue base of the flame of a candle.

There can no longer be any doubt that the nucleus consists, in its inner portions at least, of vapour of some kind, and we must now add incandescent vapour; nor does there appear to be any reasonable doubt that in most comets this vapour consists of or contains some volatile compound of carbon, unless it be carbon itself vaporized by the heat of the sun. Whether we are to attribute the bright bands to a compound of carbon or to carbon itself, is a point which has been a good deal debated, and into which I forbear to enter, though I cannot help entertaining an opinion.

To account for the telescopic appearance of the head and envelope, we must I think with Sir John Herschel admit the existence of a repulsive force emanating from the sun, a force perhaps exerted, not on the incandescent vapour itself, but on the highly tenuous cloud resulting from its condensation. According to the view which I would here present to you, the tail consists of mist of exceeding tenuity continually streaming away from the head under the influence of the repulsive force and passing into space, and continually renewed by condensation of fresh vapour which is continually ejected from the nucleus, so long as the comet is sufficiently near the sun. There is no whisking round of the tail as the comet moves round near perihelion, because the tail, though material, does not consist of the same matter, but of matter which is continually renewed.

Suppose a fire engine and a fireman with a hose mounted on a platform which is capable of revolving round a vertical axis, and suppose the man always to direct the hose so as to send a stream outwards from the axis when the platform is at rest. Let the platform now be moved round. Then the outward flowing stream will be curved with its convexity foremost, not on account of the resistance of the air, or at least very little on that account, but because the velocity of a particle of water in a direction perpendicular to the radius vector is such as to allow equal areas to be swept by the radius vector in equal times, and the angular velocity of the radius vector becomes less and less as the particle goes outwards.

The fan-shaped tails which comets sometimes exhibit is easily explained on this view by the occurrence of different jets of vapour from the nucleus, which harmonises very well with the telescopic appearances. If the velocity of ejection be not very small compared with the velocity of the nucleus in its orbit, the velocity and direction of motion of the vapour which parts company with the nucleus will be sensibly different from that of the nucleus, and the portion of the tail which is due to such a jet will be modified accordingly.

This theory so far leaves two things unexplained—the source of the very high temperature at such a distance from the sun required to render the vapour self-luminous, and that of the repulsive force.

As to the former, it may be that the emissive powers of bodies, until very high temperatures are reached, have been much overrated, from the circumstance that the experiments from which the conclusions were deduced were made in air or other gas, either at ordinary pressures or at rarefactions short of those very high ones which are now reached. Thus Kundt found that the rate of cooling of a heated wire in a vessel filled with air or other gas, when examined at different pressures, decreased indeed as the exhaustion proceeded, but at a very moderate exhaustion became sensibly constant, and remained so up to the highest exhaustion he was able to obtain. He naturally concluded that he had reached the rate due to radiation alone. But Mr Crookes, whose skill is so great in producing excessive exhaustions, while he verified Kundt's result up to very high exhaustions, found that on going still further onwards the rate of cooling began rapidly to diminish, nor with all his skill in exhaustion did he succeed in getting to a second limit where it remained constant again, this time doubtless at the rate which would be due to radiation alone. Now the planetary vacua probably far surpass what we can produce in the laboratory, and it may be that up to a very high temperature the emission of radiant heat from a small body like the nucleus of a comet, with perhaps no atmosphere but what was produced by evaporation and was passing off into space, is a good deal less than we should have supposed from laboratory experiments.

But perhaps a more probable view is what may be called the green-house theory. The explanation of the warmth of a green-house as is well known depends on the different behaviour of the radiant heat from the sun and of that which comes from the plants, stands, etc., in the green-house, with reference to their passage through glass. The former is mainly heat of high refrangibility, which passes freely through glass; the latter is mainly heat of low refrangibility, with respect to which glass is opaque. Accordingly the house gets warmed much above the temperature it would have if glass had the diathermanous properties of rock salt. It is even said that water may be boiled by putting it in sunshine under a series of glass enclosures.

Now it is conceivable that if the nucleus of a comet be endowed with an atmosphere, or perhaps even coated with a liquid, having in a high degree the combination of the transparent and athermanous characters of glass, its temperature when exposed to radiation from the sun might rise much above what we might have expected *a priori.*

As to the supposed repulsive force, Sir John Herschel in his Cape Observations has thrown out a conjecture to which recent researches have contributed increased probability. He suggests that the sun may possibly be a permanently charged electrified body, and that the condensed vapour from the nucleus, or at any rate belonging to the comet, may be charged with electricity of the same name, and may therefore be repelled. Now Mr Crookes found that in one of his highest vacua a pair of very small gold leaves made to diverge by a small charge of electricity remained diverging, apparently to the same extent, for a whole year. When we reflect what a very small charge of electricity such a pair of gold leaves could hold, we are led to the inference that a perfect vacuum, that is, a space containing no *ponderable* matter, would be a perfect insulator. There is nothing therefore unreasonable in supposing that the sun may be a permanently charged body.

On the other hand the well-known connexion of cumulus clouds with thunderstorms, and the evident formation of cumuli from the precipitation of vapour consequent on the cold produced by the expansion of ascending columns of heated air charged with moisture, seem to leave little doubt that in our atmosphere rapid condensation of vapour is as a matter of fact connected with high electric manifestations, whatever may be the cause of the connexion. It is no violent supposition therefore to make, to suppose that the condensation of the vapour coming from the nucleus of a comet may cause a similar development of electricity.

Of course if matter from a comet is thus driven off into space, the comet must gradually waste away, however slowly, so far at least as such portion of it as consists of matter thus vaporizable and driven off into the tail is concerned, after which the residue might be compared to the coke in a gas retort; and as it no longer presents the large volume due to the volatilized vapour, it would remain invisible from its extreme smallness. A very small quantity of matter might suffice for the display at each revolution, and besides we do not know how long a comet has been appearing as such. It may have been circulating for ages round the sun a long way off, until at last it passed, casually as we might say, near a planet, the attraction of which gave it a new orbit altogether, and brought it within roasting distance of the sun when near its perihelion, when it assumed its cometary character. This theory would account for the great eccentricity of the orbits of comets; for the perturbation produced by a distant planet, such as Uranus or Neptune, to which in circulating round the sun it at last made a near approach would tell far more on the direction of motion than on the velocity, so that when the body got near the sun, its velocity would usually be a little, and only a little, less than that in a parabolic orbit.

LECTURE IV.

Red prominences seen about the sun in total eclipses—Inferences as to their character derived from an examination of their light—Mode of viewing them independently of an eclipse—Evidence they afford of gigantic commotion—Corona—Alteration in the pitch of sound produced by motion of the source of sound, or of the observer—Analogous alteration of the refrangibility of light—Indications thus afforded of motions of approach or recess of the stars relatively to the earth—Indications of commotion in the sun—Application to the discrimination between dark lines of solar and of terrestrial origin in the solar spectrum—Views which we are led to entertain as to the constitution and history of the sun and stars—Conclusion.

IN the applications of the spectroscope to the heavenly bodies which I have hitherto brought before you, with the exception of those relating to the spectrum of the sun taken as a whole, one difficulty to contend with has been the feebleness of the light. I come now to a research where the difficulty has in so far been in the opposite direction as that it was due to the excess of light mixed up with that which it was desired to observe.

The astronomers who observed the total solar eclipse of 1842, which was visible in Italy, noticed particularly a phenomenon which they had not at all expected. They were fully prepared to observe the corona, but they were surprised to observe in addition to this three or four rose-coloured prominences like luminous mountains surrounding the dark disc of the moon. The duration of the total phase in this eclipse being rather short, and the phenomenon unexpected, naturally not much could be done on that occasion towards investigating their nature. But a highly interesting subject for study in future eclipses of the sun was pointed out.

With reference to the nature of the prominences, one important point to decide of course was whether they belonged to the sun or the moon. The decision of this was not difficult, for if they belonged to the moon they would retain the same positions relative to the lunar disc all through totality, whereas if they belonged to the sun they would be covered or uncovered, as the case might be, by the advancing moon. The latter proved to be the case.

In the total eclipse of 1851 a daguerreotype of the prominences was taken by Dr Busch, but it does not appear to have been good. In 1860 a total eclipse was to be visible in Spain, and the Admiralty lent a vessel to convey the observers. Among them, Mr De La Rue, who had been so successful in celestial photography, went prepared to take a photograph of the totality. Not knowing what might be the actinic power of the prominences, he thought it safest to give the whole time of totality, which was under two minutes, to one photograph, or rather pair of photographs, lest in aiming at catching two or more phases the time should be too short to impress the plates. The event proved that a much shorter time of exposure would have sufficed; for the actinic power proved to be very great, a conclusion in itself of much importance at the time. The strength of the actinic power was shown among other ways by the pictures exhibiting a prominence in the form of a detached cloud, which if not invisible to the eye had at any rate not been noticed by the eye-observers during the brief time they had to scrutinise the phenomenon.

Another observation of importance at the time as bearing on the nature of the prominences is due to M. Prazmouski, who had made well-devised preparations for determining the polarization, if any, of the prominences and corona separately. He found that the corona was strongly polarized in a radial direction, though not so strongly near the sun as a little way off. It follows that the light of the corona must, in part at least, be reflected light, and that its real seat must be round the sun, not round the moon. The light of the prominences on the other hand was altogether unpolarized. The most natural conclusion would seem to have been that they were self-luminous, though Prazmouski drew another, which does not seem to have been altogether sustainable.

Total solar eclipses do not occur very frequently, perhaps on an average once in two years. Unlike eclipses of the moon, the total phase is visible only along a narrow strip of the earth's surface, perhaps 100 miles broad or less, and this strip may be on the ocean, where observers have no firm footing for their instruments, or if not that it may be in some distant and out-of-the-way part of the earth. And even if observers make a long journey on purpose, taking their chance for absence of cloud at the critical moment, or if the total phase happens to be visible nearer home, there is only a very short time, seldom exceeding four minutes, in which to make all the observations which demand totality. Hence so long as we are confined to total solar eclipses for our knowledge of the prominences, our progress in the study of their nature must necessarily be slow.

.Mr Lockyer had for some time been engaged in solar study, examining more particularly the spots with a spectroscope, when he published the suggestion that possibly the spectroscope might enable us to detect the presence of the prominences independently of a total eclipse of the sun. Such a result might be hoped for if their spectrum, which had not then been examined, should give bright lines. The reason of this is easily understood. If we double, treble, &c. the dispersion of a spectroscope, the width of the slit remaining the same, the same quantity of light as before goes to form a given portion of a continuous spectrum. But this is of two, three... times the length it was before, and therefore the brightness is only one-half, one third... as great as at first. But if besides the continuous spectrum there be a source of strictly monochromatic light, the image of the slit as seen by this, forming what we call a bright line in the spectrum, is not more spread out by the increase of dispersion, and therefore as we increase the dispersion, the bright line continually gains in brightness relatively to the continuous spectrum. Now in attempting to make out the presence of prominences by the bright lines of their spectra, if such they have, we encounter a continuous spectrum due, partly to the solar corona, partly to the light dispersed in our own atmosphere, light which we all know is very bright in the immediate neighbourhood of the sun.

Mr Lockyer tried repeatedly to detect the prominences with his instrument, but failed. Another spectroscope of greater power was then ordered. Almost immediately after this was received, success crowned his efforts. On the 19th of November 1868 he succeeded by this method in detecting a prominence by the occurrence of the bright line C in the spectrum of light taken from immediately outside the sun in one part of his disc. Shortly afterwards a prominence was detected by the bright line F as well as C. An account of this discovery was communicated next day to the Royal Society.

Meanwhile M. Janssen had gone out to India to observe a total eclipse to be visible there on the 18th of August of the same year. He went specially prepared to examine the spectrum of the prominences. He was favoured with clear weather, and found that the spectrum in question was a bright-line one, indicating the presence of incandescent gas. The observation seemed to him so easy that it occurred to him that it might be possible by the application of the same method to make out prominences on any day if there were any present; and accordingly next day he tried the thing and succeeded. This result was not communicated by telegraph, nor did the account of it reach Europe until after Mr Lockyer's discovery had been communicated to the Royal Society and to the French Academy.

In this mode of examining the prominences, the prominence can only be made out piecemeal, by taking different sections of it by the slit of the spectroscope, observing the portion of the slit to which the bright line as seen in the spectroscope corresponds, and then putting all the sections together. Shortly after the announcement of Mr Lockyer's discovery, Dr Huggins modified the method by widening the slit sufficiently to take in a whole prominence, and preventing the drowning of the prominence by the light thus let in by using a suitable absorbing medium; actually by the use of a red glass. This method, or else that of the narrow slit, is now used habitually, and daily observations, weather permitting, of the prominences are now regularly made.

Of course total eclipses still afford the best opportunity for scrutinizing the spectra of prominences. They are found, especially in the lower parts, to show bright lines due to several metals, but the hydrogen lines are the most characteristic, and it is by means of the hydrogen line C in the red, or else by F in the blue, that the forms of the prominences are made out on ordinary days.

One of the most astounding things connected with these prominences, now that we are able to study them at any time, is the enormous rate at which they are developed. They are shot forth from the body of the sun with velocities going sometimes up to 100 miles per second, or even beyond. Their most usual forms give the idea of the ejection of a gas, which gradually is brought down again by gravitation towards the surface of the sun.

We appear to know less about the corona than about the prominences. At any rate till quite lately, we were dependent for all our knowledge about it on such observations as could be taken during the brief duration of total eclipses, in which there were other things besides the corona that claimed attention. I said till quite lately, because now Dr Huggins appears to have succeeded in obtaining photographically some representation of it independently of eclipses. The great difficulty was of course to eliminate it from the glare due to our own atmosphere that is always seen close to the sun. The preliminary trials, when the pictures were carefully examined, seemed to leave little doubt that the corona was really depicted on the photographs, though of course on account of atmospheric glare the pictures must be much inferior to what can be got during total solar eclipses. The corona has to be picked out, as best may be, from the effect of the glare by delicate differences of intensity. Of course the great object to aim at in these photographs is to obtain if possible a continuous history of the changes which may take place in the form of the corona; for though the pictures which can be taken during totality in an eclipse are far superior for the form of the corona at the moment, the occasions on which they can be taken are too rare to furnish us with much of a history.

As to the character of the light of the corona, its polarization as already mentioned shows that the light is in part at least reflected. Spectroscopic observation shows, besides a few bright lines in the part near the sun, a peculiar line agreeing in position with the brightest line of the aurora, and extending higher up. This shows that the corona is to some extent self-luminous, especially near the sun, which falls in with Prazmouski's observation that the polarization appeared stronger a little way from the sun than close to the disc. There is however a circumstance independent of self-luminosity which would tend towards the same result. What the particular substance or condition of a substance may be that gives rise to the green line common to the corona and the aurora, requires further elucidation.

In my course of lectures last year I frequently referred to the phenomena of sound as illustrating, or suggesting the explanation of, certain phenomena of light. In the present year's course I have not hitherto referred to sound; but I now come to a phenomenon regarding light for an easier apprehension of which it will be convenient to consider the analogous phenomenon in sound.

Suppose that a person is standing on the platform of a railway station when an express train passes by at full speed, and that the engine driver keeps the whistle sounding as the train rushes past. If the observer on the platform notices the pitch of the whistle, he will find that there is a very sensible fall of pitch when the train has passed. The same thing would be observed still more strikingly when two trains moving in opposite directions pass each other at full speed, the observer being in one, and the engine driver of the other keeping the whistle sounding as the trains pass.

Now the whistle is the same all along, and worked in the same way. What then is the cause of the lowering of pitch?

The pitch of a musical note depends we know on the frequency of the vibrations, on the number, that is, of pulses per second which strike the ear. When that number is doubled, the pitch is an octave higher, when halved, an octave lower, and the ratio for other musical intervals is well known. Suppose now that a sustained series of vibrations proceeds from a source of sound *W*, and the sound is heard by an observer *O*, the air being quite calm at the time, and the source *W* and the observer *O* being at rest. Then it is easily seen that the number of pulses per second which emanate from W is just the same as the number which strike the ear of the observer; the effect of the finite velocity of propagation of sound is merely that the individual pulses emanating from W do not affect the observer until the expiration of the time that sound takes to travel from W to O.

If the wind be blowing, the result is still the same, as we see at once if we suppose the vibrations to go on indefinitely, since just as many vibrations as start from W must reach O. The pitch therefore of the note is not altered by the wind.

But it is otherwise if the source W be moving towards or from the observer. Suppose that in any given short time W moves to W', in the line WO, in proceeding towards O with the velocity v, and let V be the velocity of sound in still air. As the source travels from W to W', it is continually sending out vibrations at the normal number per second corresponding to the pitch of the whistle or other source, and these travel towards O with the velocity of sound. Had all the pulses started from W' at their due intervals of succession, they would have been received at O at the same intervals apart; but as it is, those that start from the source on its journey to W' are handicapped by the time sound takes to travel to W'. Hence whereas the whole time occupied by the issue of the series of vibrations is that the source takes to travel from W to W' with velocity v, the whole time occupied in the reception of the vibrations at O is less than the former by the time sound takes to travel from W to W' with the velocity V. Hence the ratio of the durations of issue and reception of the same series of vibrations is that of V to $V-v$; and since the ratio of the frequencies, or numbers of pulses per second, is the inverse of the former, the frequency of reception will be to the frequency of issue as V to $V-v$. The pitch of the note as heard at O will accordingly be raised by the motion of the source towards O by the quantity corresponding to the above ratio.

In a precisely similar way, if the source be receding from O instead of approaching it with the velocity v, the frequency of reception will be to the frequency of issue as V to $V+v$, and the pitch of the note will be lowered accordingly. And if the source be first approaching to and afterwards receding from O with the velocity v, the ratio of the frequencies as the source approaches and recedes will be that of $V+v$ to $V-v$.

Thus supposing the source to be the whistle of a train travelling at the rate of 45 miles an hour or 66 feet per second, and taking 1100 feet per second for the velocity of sound, we have for the ratio of the frequencies in approach and recess 1166 to 1034, or

1128 to 1000, nearly. This slightly exceeds the ratio, 9 to 8, or 1125 to 1000, which is that for a major tone. The effect therefore is quite palpable, and would be easily perceived even though the train were travelling a good deal more slowly.

If instead of the source moving towards the observer, the observer is moving towards the source with the velocity v', the source itself being at rest, a little consideration will show that the effect is the very same, and the ratio of the frequency of the sound heard to the frequency of issue is that of V to $V-v'$; and if the source and the observer be both moving towards each other with velocities v, v' respectively, the ratio of the frequency as heard and as issued will be that of V to $V-(v+v')$. This does not necessarily suppose that v and v' are very small compared with V; and the formula will apply to all cases whether of approach or recess, if we suppose v or v' to be negative when the source or the observer is receding instead of approaching.

Now if light consists in undulations, the same thing ought to occur with it. The velocity of light, about 186000 miles per second, is however so enormous, that any velocity we can mechanically produce on earth is too insignificant in comparison sensibly to affect the frequency of the vibrations reaching us, or consequently the refrangibility, which depends on the frequency. But in the earth and heavenly bodies we have masses moving with velocities which are not incomparably smaller than the velocity of light. It is conceivable therefore that some indication of a variation of frequency should be capable of detection in connexion with these motions. Thus the earth in its motion round the sun moves in round numbers 20 miles in a second, and accurate measurements of the positions of the stars show that many of them have what is called proper motions; that is, individual stars show small progressive changes of position relative to the body of stars taken as a whole. And since on the whole there appears to be a slight opening out of the stars on one side of the heavens, especially in the neighbourhood of the constellation Hercules, and a closing in on the opposite side, it has been concluded that the whole solar system is probably moving in space in the direction of that constellation.

The only motion of the earth and the stars of which we can thus take cognizance is that part of the relative motion of the earth and any particular star which is transverse to the line joining them. And as the displacements observed are all angular displacements, displacements, that is, of the direction of the joining line, we can only draw a conclusion as to the relative velocities in a transverse direction on condition that we know the distances of the stars. The distances of the stars are so enormous that it is only in some cases that astronomers have been able to determine them roughly with some degree of confidence, through examination of the annual parallax. It has been concluded as probable that our own system is moving relatively to the stars as a whole with a velocity something like that of the earth in its orbit round the sun.

Inasmuch as in contemplating the stellar universe our own sun is merely one particular star, taken at random, there is no reason why the direction of motion of a star in space should have any particular relation to that of the line joining the star with the sun. If then motions in a direction perpendicular to the joining line be revealed by astronomical observation, the probability is that motions also exist in the direction of the joining line, though of these no cognizance can be taken by observations of the positions of the stars; and if velocities of approach or recess of the stars relatively to the sun exist, it may be that they might admit of detection by the change of refrangibility which they would occasion.

In the illustration I gave of the railway whistle, the effect of the motion of the train on the pitch of the sound as heard by the observer was detected by the sudden change from a higher to a lower pitch as the train passed by. We have no such means available in the case of a star. How then would a change of refrangibility, if it exists, admit of detection?

To go back to the railway, suppose the driver of the approaching train were to shut off the steam from his whistle before he reached the station; how in that case could the observer on the platform detect a change in the pitch of the whistle as heard by him from what it would have been if the train had been at rest? Only by comparing the note given by a whistle, or similar instrument which he carried about him, and which had previously been adjusted to unison with the same railway whistle when the train was at rest, with the note heard when the train is approaching the station.

Now can we find two kinds of light, of definite refrangibility, one on earth and one in a star, which we know to be of equal frequency at issue?

I have already referred to the remarkable spectrum of Sirius, and the overwhelming evidence which the coincidences of so many of its dark lines with the bright lines in the spectrum of incandescent hydrogen afford of the existence of hydrogen in the star. To this substance would correspond definite frequencies of vibration whether it vibrated on earth or in Sirius, assuming of course that the same laws of matter exist in that distant star as on earth. But if the frequencies of issue were the same, the frequencies of the light as received on earth ought not to be rigorously the same, if the relative velocity of approach or recess of the earth and Sirius be not insensibly small compared with the velocity of light.

Now on comparing with all possible accuracy the positions of the dark line F in the spectrum of Sirius with the bright line F in incandescent hydrogen, Dr Huggins found that the coincidence was not quite perfect; the centre of the bright line F lay towards the more refrangible edge of the dark line, or rather very narrow band, in the spectrum of Sirius.

Now assuming that the frequency of issue was the same in the two cases, such a result would be produced by a suitable velocity of recess, and we might deduce that velocity from a measurement of the amount of displacement. The displacement is so excessively small that in any attempt at measurement large allowance must be made for the inevitable errors of observation. Still a tolerably satisfactory result was arrived at, and it appeared that at the time of the first series of observations the relative velocity of recess of Sirius from the earth was about 41 miles per second. Deducting from this the portion due to the motion of the earth in its orbit, there remained about 29 miles per second as the velocity of recess of Sirius relatively to the sun.

The path thus opened out has been followed up by Dr Huggins himself and by others; and comparisons between the dark lines of stars and artificial bright lines, with a view to determine the longitudinal components of the motions of the stars relatively to our sun, are now regularly carried on at the Royal Observatory, Greenwich.

It may appear at first sight illogical to draw conclusions as to the existence of such or such an element in a star from the coincidence of certain lines in its spectrum with bright lines in artificial sources, and then to draw further conclusions from the imperfection of the coincidence. This however is not so. If we could affirm that the coincidence ought to be mathematically perfect on the supposition that the substance in the star was the same as what we have on earth, then indeed a defect of coincidence would be fatal. But if on the contrary we can assign a cause why the coincidence should not be perfect, especially if the conditions assigned for the non-coincidence are far more likely to be present than not, then the case is very different. Then, with one important exception, the evidence for the existence of the substance in the star is the same as it would be if the coincidence were observed by a less perfect instrument in which the errors of observation would prevent us from being able to affirm with the same exactitude as with the better instrument that there was or was not a coincidence; but the less exact instrument might still leave the coincidences far too striking to be attributable to chance.

The exception referred to is, that the explanation of non-coincidence by relative motion leaves but one disposable constant whereby to account for the defects of coincidence, so that there ought to be a relation between the defects in the different parts of the spectrum, following an assignable law. No such discrepancy between the results obtained from different parts of the spectrum as would lead us to reject the explanation of imperfection of coincidence by proper motion has hitherto been shown to exist.

The alteration of refrangibility due to relative motion has been observed on a body nearer home—on our own sun. Mr Lockyer has specially studied the behaviour of spots and their neighbourhood with respect to spectral lines, and has found instances in which bright lines, such as the C or F line of hydrogen, have actually been seen on the disc of the sun. Moreover the lines in spots and their neighbourhood, whether bright or dark, are sometimes much contorted, indicating extraordinarily violent up or down motions of the gases. A velocity as great as 140 miles per second has thus been observed through the attendant change of refrangibility. The general phenomena seem to indicate that the bright lines belong to portions of intensely heated gas which have rushed up from the interior of the sun, from what depth of course there is no way of saying, while the spots are due to the absorption of light from below by cooler, though still intensely hot, gases which are commonly descending.

Before I conclude I must briefly refer to a singularly elegant method which M. Cornu has devised for distinguishing between those dark lines of the solar spectrum which owe their origin to the sun and those which are due to absorption of the sun's light by the atmosphere of the earth. He places a lens mounted so as to admit of a rapid lateral oscillating motion in front of the slit of the spectroscope, which for this purpose must be one of very high dispersion, at such a distance as to form an image of the sun on the slit. The amount of lateral motion is such as to bring the two lateral limbs of the sun, near the edges, so as to fall on the slit alternately. The telescope is so mounted that the slit is, roughly at least, perpendicular to the sun's equator.

Now in consequence of the sun's rotation, from west to east, round an axis, his eastern limb is moving towards us, and the western limb from us, with a velocity of about 1¼ mile per second. The difference of refrangibility between the two for light of a given frequency of issue would be that corresponding to a velocity of approach or of recess double of the above, and the corresponding displacement would not be insensible in a spectroscope of very high dispersion. Now when the lens is made to oscillate, the eastern and western limbs are brought on the slit alternately in quick succession, and the consequence is that there is a slight lateral oscillation in the lines which are of solar origin. But there is no difference made as regards the lines that are due to absorption by the earth's atmosphere, since as regards them the solar light is tantamount to light furnishing a continuous spectrum, and the absorption takes place for light of a given frequency of reception, not frequency of issue. Accordingly the lines of solar and those of terrestrial origin are distinguished at once, simply by looking at them, the former oscillating a little right and left, while the latter remain fixed.

Let us pause a little here to consider the views which our present knowledge opens out to us as to the constitution of the sun.

We have seen that the interpretation of the dark lines of the solar spectrum leads us to regard even the outer portions of the sun's atmosphere as still intensely hot, so much so that even such refractory substances as iron, chromium, &c. are kept in a state of vapour. If such a temperature be maintained, notwithstanding the enormous and constant loss of heat from the sun by radiation into space, the body of the sun must be hotter still. It must therefore be at a temperature great enough to keep iron, chromium, &c. in a state of vapour, unless it be that they are liquefied by pressure notwithstanding the higher temperature. Dr Andrews has shown that for such liquids as liquefied carbonic acid, ether, &c., and presumably for all liquids, there is a critical temperature, differing from liquid to liquid, above which there is a perfectly continuous passage, as the pressure is increased, from what everybody would call gas to what everybody would call liquid. It may be that the temperature of the sun is above the critical point of iron and other known elements which are ordinarily considered highly refractory. It may be that at the intense temperature of the body of the sun iron is dissociated, and resolved into still more elementary forms of matter, while in the neighbourhood of the surface it is re-formed by the combination of elements of a higher order. The results of constant observations on the iron lines of the solar spectrum under varying conditions such as spots, date of observation, &c., and a comparison of these results with those relating to the bright lines of iron produced by different forms of the electric discharge, have led Mr Lockyer to be in favour of such a view. But as regards the maintenance of the energy which is continually lost from the surface of the sun by radiation into space, it signifies little whether we suppose the transition from the vapour of iron in the superficial parts of the sun to molten iron at a higher temperature in the parts lying more deeply to be continuous or abrupt, or again whether we suppose that iron as such, whether in the state of vapour or as one constituent of a molten mass, exists throughout, or only in the parts near the surface, being formed from more elementary kinds of matter in the deeper, hotter parts. The same applies to the other substances which we call elements. On any of these suppositions the general process of maintenance would be the same; the intensely heated, outer portions which radiate into space must get somewhat cooled thereby; and presently the outer strata getting cooled as a whole by the accumulation of the effects of convection currents on a small scale, we have an interchange on a grand scale between the cooler strata above and more intensely heated matter from below. These uprushes of most intensely heated gas form the prominences which are traceable round the edge of the sun, and they are sometimes indicated even on the full disc by the exhibition in the spectroscope of lines which are seen as bright in spite of the intense luminosity of the disc. The downrushes of the gases which though absolutely intensely hot are relatively cool, appear as dark spots or patches on the sun's surface, in consequence of their absorbing action on the light radiating from the hotter parts below, which they do not make up for by their own inferior radiation. These interchanges on the grand scale between the more and less intensely heated gases appear to be subject to a rough periodicity, the period being about 11 years. But interchanges on a smaller scale are always going on, analogous to the small convection currents in air which are always taking place near the surface of the earth when the sun is shining. With a sufficiently high magnifying power and sharpness of definition, the sun's surface is seen to be dotted over with minute black spots called pores, and between these are small brighter patches, the same apparently as the willow leaves of Nasmyth. These are admirably depicted in the large-scale photographs of M. Janssen; and so rapid are the changes that two photographs taken at an interval of even a few seconds are found not to be identical. So constant is the state of turmoil in the central luminary of our system, which is like a huge boiling pot, nearly a million of miles in diameter, only that the currents of convection arise from cooling at the top instead of heating at the bottom; and were it not for that, did the interchanges take place only at considerable intervals, the light and heat of the sun would be subject to extensive and fitful changes, prejudicial to the welfare of the living beings which inhabit our earth, and it may be other planets of our system. Thus everywhere we behold that mutual adaptation of the various parts of the system of nature which leads us to the conception of a designing mind.

The radiation from the sun is the proximate maintainer of processes which are essential to life, whether animal or vegetable, as I hope to explain more fully in my concluding course of lectures; and as regards this the most recent discoveries teach us more and more that we are living upon capital. The sun is a vast bank of energy, but the supply, though it may last for ages, is not inexhaustible. Progress, not periodicity, appears to be the order of things when we contemplate them on the great scale. The present order of physical nature cannot be pushed back to a past eternity, nor is it calculated to remain as it is for an eternity to come. It may be that in the planetary nebulae we are permitted to witness an early stage in the process of creation. Even when stellar points exist in it, we can hardly suppose that the system is adapted to the habitation of living organized beings. Bathed in a fiery gas, of fiercer temperature than the oxyhydrogen jet, though that melts platinum like sealing wax in a candle, the system could not support life at all analogous to what we have on earth, nor would it be adapted for such a purpose for ages to come. In the red stars on the other hand, with their peculiar spectra, it may be that we have systems which are either already effete, or which are in process of becoming so. The indications of the history of the universe which modern science has disclosed lead us to the contemplation of durations of time as vast in their way as are those distances in space with which we have long been familiar, from the result of astronomical measurements as applied to the solar system and the starry heavens.

The subject of my present course of lectures has carried our contemplations to the boundaries of the visible universe, and has given us fresh evidence of a similarity of plan running throughout, combined with individual difference of feature. The difference between the spectra of different stars is analogous, as Dr Huggins and Dr Miller have remarked, to the difference of materials between one locality on the earth and another. Just as in certain places some particular chemical element is found in quantity, though in general it may be rare, so in some of the stars we have abundant evidence of the presence of elements which in the earth, at least in the portions close to the surface, which alone are accessible, and apparently in the sun too, occur but sparingly. We can hardly suppose that life is confined to one particular planet circulating around one particular sun out of this vast multitude. In face of the views that thus open out to us, the feeling of the littleness of man comes upon us with almost overwhelming force, so much has modern research emphasised those words uttered of old, "What is man that thou art mindful of him?" But when from the contemplation of such immeasurable distances we turn to an individual living being, when for example we consider the structure of our own bodies, and the wonderful adaptation of the various organs to their purpose, we see that the vastness of the universe has not caused the Creator to be unmindful of the least of his creatures. On some of those adaptations, in which light is concerned, I hope to dwell in my remaining course. It is rather on the vastness of the scale and yet unity of plan of the universe that this year's course has led us to ponder. And to pass from material construction to moral government, we are warned by the contemplation of material nature to expect a moral government on a vast scale, and carried out in obedience to general laws, which if we disregard we must take the consequences.

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